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ART. XXXVIII.—*The Higher Oxides of Copper;* by THOMAS B. OSBORNE.

Copper dioxide.—When copper hydroxide is treated with hydrogen dioxide at a temperature near 0° C. a brown precipitate is formed which has been shown to contain nearly twice as much oxygen as copper oxide (CuO) and when acidified yields hydrogen dioxide. When the temperature is somewhat higher a grass-green precipitate is formed which is sometimes yellow-green or yellow. Krüss (Berichte, 2593, 1884) has shown that the brown precipitate, when treated at 0° C. for some days with excess of hydrogen dioxide and frequently shaken, can be filtered and washed first with alcohol and then with ether and when entirely freed from water can be dried at 100° C. It then has the composition of CuO_2, H_2O or H_2CuO_3. When moist Krüss found that it began to decompose above +6° C. Above this temperature he obtained a grass-green precipitate which began to decompose above +12° C. He was unable to analyze this latter as it decomposed readily.

Brodie has shown that if hydrogen dioxide be added to a salt of copper in solution and then soda, a yellow precipitate is formed.

Krüss obtained a yellow precipitate in too small quantity to analyze, by fusing copper oxide with sodium chloride with access of air. He thought this was a higher oxide of copper

and anhydrous, and suggested that it might be the same as Brodie's oxide. Having been unable to find any investigations of the composition of these variously colored precipitates I thought it worth while to try and straighten them out. I found that if copper hydroxide (prepared by precipitating nitrate of copper with soda and washing thoroughly with cold water by decantation so as to keep the copper hydroxide in a state of the finest possible subdivision) was treated with hydrogen dioxide in small amounts, a green precipitate was formed which by further action of hydrogen dioxide was changed into a brown. This brown precipitate was yellow by transmitted light but dark brown by reflected light. When a small amount was shaken up with water it appeared yellow. I found that by mixing increasing amounts of hydrogen dioxide with copper hydroxide the color of the precipitate varied from a yellow with a greenish tinge, through yellow to brown. I made a number of preparations thus varying in color and after washing by decantation free from hydrogen dioxide determined the hydrogen dioxide set free by dissolving the precipitate, suspended in water, with dilute sulphuric acid. By using a solution of potassium permanganate of known strength the hydrogen dioxide set free was easily determined, the copper in the solution was precipitated with hydrogen sulphide and determined, and the ratio between the excess of oxygen found by titration, and the copper oxide calculated. In this way I found the following ratio between the oxide of copper and the oxygen for the different colored precipitates with the exception of I, which was determined by filtering and washing the precipitate with water, alcohol and ether, cooled to 0° C., drying at 100° C. and then heating in a tube in a current of dry air, weighing the water and copper oxide and calculating the oxygen by difference.

		Per cent.	Weight.	Ratio.
I. Grass green	CuO	82·35	·6679	12·2
	H_2O	16·29	·1322	
	O	1·36	·0110	1·
		100·00		
II. Grass green	CuO		·2717	12·7
	O		·00422	1·
III. Yellow green	CuO		·3040	7·5
	O		·00814	1·
IV. Yellower green	CuO		·0701	5·14
	O		·00275	1·
V. Yellow (green tinge)	CuO		·4757	4·2
	O		·02271	1·
VI. Yellow	CuO		·0633	3·1
	O		·00413	1·

VII.	Yellow brown	CuO	·2018	1·66
		O	·02457	1·
VIII.	Brown (yellow tinge)	CuO	·2847	1·48
		O	·03893	1·
IX.	Brown	CuO	·2813	1·2
		O	·04669	1·

From these analyses I concluded that the green precipitates were simply mixtures of the yellow with the blue and that the yellow was a mixture of the brown and the green, the reddish brown color of the dioxide of copper being complementary to the green and thus destroying one another and leaving the yellow predominant. That these different colors are mixtures of CuO_2, H_2O and $Cu(OH)_2$ was further supported by the fact that I could reproduce any shade of color by mixing at any temperature the brown CuO_2, H_2O with $Cu(OH)_2$ in proper proportions. Furthermore the fact that all yielded hydrogen dioxide when dissolved in dilute acids without evolution of oxygen is evidence in support of this view.

I found further that I could filter, wash and dry these precipitates under the same conditions that Krüss employed for CuO_2, H_2O; and analysis by heating in a tube and weighing the water and copper oxide gave results similar to those obtained by determining the hydrogen dioxide set free by acids, as is seen by I which was determined in this way while II was obtained by the permanganate method. I did not find that any of these precipitates decomposed at low temperatures when free from hydrogen dioxide. Krüss states that the brown precipitates begin to decompose above +6° C. and the green above +12° C. I found that when hydrogen dioxide was present there was a slow evolution of oxygen even below +5° C. which increased as the temperature rose. The precipitate however underwent no apparent change until boiled for a moment or two, when it suddenly yielded a large amount of oxygen and black CuO separated out. It would appear then that the decomposition below 100° C. is of the hydrogen dioxide, brought about by the action of the copper dioxide.

From the fact that no definite composition can be assigned to any of these various colored precipitates except the blue copper hydroxide and the brown copper dioxide, that they all yield hydrogen dioxide with acids, that they show the same behavior on heating, that the different colors can be produced by different amounts of hydrogen dioxide with the same amount of copper hydroxide and that the colors may be reproduced by mixing the copper hydroxide and the copper dioxide, it seems reasonable to conclude that hydrogen dioxide forms but one higher oxide with copper.

Further the yellow oxide obtained by Krüss by fusing cop-

peroxide with sodium chloride cannot be the same as Brodie's as suggested by Krüss, for the latter's compound must have been anhydrous while Brodie's is most probably a mixture of copper hydroxide with copper dioxide. When boiling a small amount of copper dioxide with water I obtained after a few minutes a bright yellow precipitate which did not decompose even when boiled for sometime, nor on standing some days in a test tube in the laboratory. There was too little to analyze and I could not obtain it again. This may have been the same oxide as Krüss obtained by fusing copper oxide with sodium chloride. Lack of time prevented me from further investigating this compound.

Copper sesquioxide.—Crum (Liebig's Annalen, lv, p. 213) described a higher oxide of copper which he obtained by mixing with one pound of bleaching powder, solution sp. gr. 1·06, 50 grains sifted calcium hydroxide, and then adding to the solution cooled to 0° C. 20 grains of copper oxide dissolved in 70 grains of nitric acid sp. gr. 1·3. In this way he obtained a blue solution from which a black precipitate separated on standing some hours. This precipitate evolved oxygen and floated on the top of the solution for sometime. After twenty-four hours or more the oxygen ceased to come off and the precipitate settled to the bottom in a dense condition. This precipitate, when finely divided, had a crimson tinge and a brighter red color when rubbed with a rod to a thin layer on glass. When soda was used instead of the suspended lime, he obtained a rose-red precipitate which would not settle. Crum considered the precipitate obtained with lime the same as that obtained with soda and attributed the difference in color to a difference in subdivision. He succeeded in washing the dense black precipitate by decanting with cold lime water and found that it contained no Cl and an excess of oxygen.

He made several determinations of the ratio between the copper and the oxygen by taking a weighed amount of copper oxide, treating it as described and measuring the amount of oxygen evolved on acidifying. For 20 grains of copper oxide he obtains in six analyses, I 1·875, II 1·886, III 1·748, IV 1·915, V 1·795, VI 1·747 grains of oxygen. The mean is 1·828 grains. Cu_2O_3 would require 1·98 grains. By re-calculating these results so as to show the ratio between the oxygen found and the copper oxide I obtained the following numbers:

I.	For one atom of oxygen,			2·14	copper oxide.
II.	"	"	"	2·15	"
III.	"	"	"	2·30	"
IV.	"	"	"	2·11	"
V.	"	"	"	2·24	"
VI.	"	"	"	2·30	"

From this he concluded that the formula of the oxide was probably Cu_2O_3 and further that this oxide acted as an acid and was united with lime, forming a cuprate of calcium, although he obtained no proof that this latter supposition was correct. I repeated Crum's experiments in making this compound of copper with the results as described by him. Hoping to obtain a more accurate determination of the oxygen I tried the following method.

After the precipitate had settled, the hypochlorite solution was poured off as far as possible and then the flask filled up with lime water and shaken in order to mix thoroughly. After allowing the precipitate to completely settle, 50 c. c. of the clear solution were drawn off and a known amount of ferrous sulphate solution run in and after standing some minutes sulphuric acid added to dissolve the precipitated ferrous and ferric hydroxides and then the excess of ferrous sulphate titrated off. 50 c. c. of the solution containing the copper precipitate were next drawn from the bottom of the flask and treated with the same amount of ferrous sulphate solution in the same way. The difference in the titer in the two cases represented the oxidation due to the excess of oxygen in the copper compound. The strength of the permanganate solution being known the weight of oxygen was easily calculated. The first 50 c. c. of clear solution drawn off were in all cases examined for copper but none was found. The copper in the other 50 c. c. was carefully determined and the ratio between the copper oxide found and the excess of oxygen calculated. Thus I obtained the following results:

A.

	Weight.	Ratio.
I.	CuO = ·1888 grams.	CuO = 2·95
	O = ·0129 "	O = 1·
II.	CuO = ·1261 "	CuO = 3·
	O = ·0084 "	O = 1·
III.	CuO = ·0861 "	CuO = 3·25
	O = ·0053 "	O = 1·

B.

Washed thoroughly by decantation.

I.	CuO = ·1524 "	CuO = 2·38
	O = ·0129 "	O = 1·
II.	CuO = ·3182 "	CuO = 2·57
	O = ·0249 "	O = 1·

C.

I.	CuO = ·2278 "	CuO = 2·83
	O = ·0162 "	O = 1·
II.	CuO = ·0716 "	CuO = 2·40
	O = ·0060 "	O = 1·

III. CuO = ·1505 grams.		CuO = 2·25
O = ·0135	"	O = 1·
IV. CuO = ·1217	"	CuO = 2·7
O = ·0092	"	O = 1·

D.

Ferrous chloride was substituted for ferrous sulphate with results as follows :

Weight.		Ratio.
I. CuO = ·1687 grams.		CuO = 2·93
O = ·0117	"	O = 1·
II. CuO = ·1614	"	CuO = 2·90
O = ·0113	"	O = 1·
III. CuO = ·0703	"	CuO = 2·4
O = ·0059	"	O = 1·

To what the irregularities in the results were due I am unable to say. The action of the hypochlorite solution on the ferrous sulphate solution was the same when equal quantities were used. No hydrogen dioxide could be found in the acidified solutions which could act on the iron irregularly and also on the permanganate, and no oxygen escaped on acidifying. I think it probable that a more extended trial of this method would yield satisfactory results.

I tried to use potassium iodide instead of ferrous sulphate but with little promise of success as the results varied greatly. I found it exceedingly difficult to obtain constant results with potassium iodide in determining cupric salts alone except in very dilute solutions so I abandoned this method as unreliable.

Of course it would be impossible to prove whether calcium was a constituent of this copper precipitate or not when made according to Crum's method, for the suspended calcium hydroxide settles down with the precipitate containing copper and could not be separated. I found, however, that a large excess of lime water could be substituted for the suspended calcium hydroxide. By filtering the cold calcium hypochlorite solution carefully into a large flask provided with a rubber stopper, with two holes, through one of which the funnel passed and through the other a tube filled with broken stick potash to exclude carbonic acid, and then running in about one liter of cold lime water, and adding the copper nitrate solution, the copper hydroxide which first precipitated, in four or five minutes went into a perfectly clear solution of a deep blue color. This solution soon changed to a green and then became nearly black, and from this a precipitate began to separate which had a crimson color by transmitted light, and appeared exactly like the precipitate obtained with barium hypochlorite instead of calcium to be described beyond. In a few minutes this precipitate began to evolve oxygen and was converted into a heavy flocculent black precipitate.

Oxygen continued to be evolved for some hours and the precipitate was kept floating on the surface. After standing twenty-four hours on ice, it had settled. A portion of the solution containing this precipitate was drawn off and washed with lime water. Under the microscope this precipitate appeared perfectly homogeneous and with a decided pink tint. The oxygen was determined with ferrous sulphate in 50 c. c. of the solution containing the precipitate as before described, and likewise the copper and the calcium. 50 c. c. of the clear solution were drawn off and found to be free from copper and the calcium determined. By subtracting the calcium found in the solution from that found in the solution plus the precipitate, the calcium of the precipitate was found. The error due to the amount of the solution displaced by the precipitate was too small to make any serious difference in the results.

In this way I found

	In sol. + precipitate.	Solution alone.
CuO =	·1869 grams	none
CaO =	·1798	·1104 grams.
O =	·0108	none

	Weight.	Precipitate. Weight.	Ratio.
CaO in p. p. + sol.	·1798	CuO = ·1869	= 23·54
CaO in sol.	·1104	CaO = ·0694	= 12·39
CaO in p. p.	·0694	O = ·0108	= ·0675

The oxygen found in this case is much lower than in the other analyses given, but from the wide variations in the former results its determination cannot be considered more than a qualitative test. The calcium it will be seen is in nearly a ratio of two atoms of copper to one of calcium, but I consider this single analysis by no means evidence of anything more than the *presence* of calcium in the compound and is so far as I can find the only direct evidence that copper ever acts as an acid.

The rest of the precipitate from which this sample was taken for analysis was allowed to stand a week packed in ice. It grew gradually more and more dense and of a brighter pink, until finally it was of a very brilliant rose red, and formed in places on the sides of the flask very thin layers of a dazzling crimson. In the hope of being able to analyze this as Krüss did the dioxide of copper, I arranged a filter packed in ice and covered with a bell jar communicating with the air through potash tubes, so as to keep out all carbonic acid, but before I could wash the precipitate it began to turn blue next to the paper. The middle portions were dried in the cold over sulphuric acid and remained quite pink when dry, but the decom

position had extended so far, I abandoned the analysis intending to try again, but have not since had time. Unfortunately lack of time prevented me from working further in this direction, and I give my results in hope that they may be of service to others in following up this subject and settling beyond a doubt the composition of this interesting compound.

When barium hypochlorite in alkaline solutions is allowed to act on copper salts in the cold, a crimson red precipitate is formed, which somewhat resembles the calcium cuprate just described, but in many respects appears different. But little seems to be known of this compound, it having been supposed to be either a higher oxide of copper, the same as the precipitate obtained with calcium, or else a corresponding barium salt. In hope of being able to analyze this red precipitate, I obtained results which I am entirely unable to account for and publish them in the hope that some one may be tempted to investigate further and reach a satisfactory conclusion.

In order to make the red precipitate I found it best to put about 50 c. c. of strong baryta water cooled to about 5° C. in a small flask, add ·2 gram of copper as nitrate or chloride, and then to pass chlorine gas through the solution till the color changed to a blackish green, then to add 100 c. c. of baryta water of 5° C., and after shaking vigorously let stand about half an hour. The precipitate which forms on adding the 100 c. c. of baryta water changes from a blackish green to a dark dirty red and then slowly to a bright crimson. This precipitate settles very slowly and evolves a gas gradually, and even when kept cold will decompose on standing a few hours. All my attempts to wash it failed utterly. On a filter paper it decomposed rapidly. When washed with cold baryta water by decantation, no apparent change beyond the very slow evolution of gas could be seen until several times decanted, when suddenly the crimson color would disappear and a dirty pinkish precipitate remain. I had to abandon my attempts at decanting more than once or twice and endeavored to determine the composition of the precipitate by decanting once, letting the precipitate settle, and then drawing off 50 c. c. of the clear solution and 50 c. c. of the solution containing the precipitate and analyzing each, the difference representing the precipitate.

No. 1.

	Precip. + sol.		Sol.		Diff. = p. p.
I. CuO	·2905	=	....	=	·2905
BaO	·6965		·6669		·0296
O	·0269		·0267		·0002
Cl	·0268		·0245		·0023
II. CuO	·3422		....		·3422
BaO	·7025		·6657		·0368

	No. 2.		
I. CuO	·3035	----	·3035
BaO	1·0754	1·0222	·0532
O	·0242	·0240	·0002
II. CuO	·3127	----	·3127
BaO	1·0837	1·0197	·0640
	No. 3.		
I. CuO	·2603	----	·2603
BaO	·5698	·5577	·0121
O	·0082	·0072	·0010
Cl	·0142	·0118	·0024
II. CuO	·1918	----	·1918
BaO	·5627	·5577	·0050
O	·0082	·0064	·0018

The above analyses show a small amount of barium in the precipitates, compared with the copper, which varies greatly with the different preparations and considerably in the different samples of the same preparation, while duplicate determinations in the solutions agree quite closely. A little chlorine was also present, but it seems more probable that the barium and chlorine were carried down with the precipitate as barium chloride than that they were united with the copper. The results show a fair average of a very large number of determinations of oxygen in various preparations. While with the calcium-copper precipitates I never failed to get an oxidation of the ferrous sulphate or chloride, with the barium-copper precipitates I never got an oxidation greater than in No. 3, II. It is evident that we have here two different compounds, but what the barium precipitate can be I cannot even suggest. I could get no evidence of an excess of oxygen in it in any way. Iodized starch paper was unchanged when placed near the precipitate moistened with hydrochloric acid; whereas chlorine was freely evolved when the calcium cuprate was thus treated. When treated with sulphuric acid there was no evolution of oxygen, while with the calcium cuprate there was always a lively evolution of gas. When strontium was substituted for barium the same changes of color took place, excepting a darker, duller red precipitate was formed. It was necessary to suspend finely ground crystals of strontium hydroxide in the water on account of its slight solubility.

With barium, strontium and calcium (when calcium hydroxide solution is used instead of suspended lime) the same changes of color take place, but in the case of the calcium the red precipitate, which is the final state with the barium and strontium precipitates, is followed by a change to a bulky black precipitate which evolves oxygen for some time and gradually settles in the solution and changes to a crystalline rose-red precipitate.

The probability of my being unable to continue this investigation, for some time at least, has induced me to publish these very incomplete results in the hope that the beginning I have made will be of use to others who may wish to follow this subject further.

Sheffield Laboratory, April 10, 1886.

Art. XXXIX.—*The structure of the Triassic formation of the Connecticut Valley;* by William Morris Davis.

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Contents: The physical problem—Limitation of acceptable hypothesis—Disturbance has taken place—Its date is after the period of deposition—It was not caused by overflow or intrusion of trap-sheets—It was not a simple monoclinal tilting—Evidence for occurrence of unseen faults—Their systematic position—Their control of the topography—Crescentic ridges—Area and depth of disturbance—Probable character of disturbing force—Its action on the fundamental schists—Consequent monoclinal faulting of overlying Triassic strata—Strike of faults determined by strike of schists.

The trap-ridges in several parts of the Triassic formation of the Connecticut Valley were examined in the summer of 1885, with the aid of Mr. C. L. Whittle, volunteer assistant, and the results obtained fully confirm the suggestion made several years ago (this Journal, 1882, xxiv, 345; Bull. Museum Comp. Zool., vii, 1883, 249; Proc. Boston Soc. Nat. Hist. xxii, 1882, 116), to the effect that nearly all these ridges are the outcropping edges of contemporaneous lava overflows, and that the outcrop of a single sheet is repeated several times by faults nearly parallel to the strike of the beds. The evidence on which these conclusions rest will be presented more fully in the Seventh Annual Report, U. S. Geological Survey. The only considerable ridges formed by intrusive trap sheets seem to be those in the lower part of the formation included in Percival's western group; but these have not yet been sufficiently examined to make it clear that they are all of this origin.

On the basis of these results, it is desired to present here in brief form an hypothesis that may account for the peculiar eastward monoclinal structure of the formation. It will be best understood by approaching it through a review of the suggestions that have heretofore been made to the same end.

It was early suggested * that the monoclinal attitude of the formation might result from original oblique deposition or cross-bedding on a large scale. Apart from the improbability of

* H. D. Rogers, Third Ann. Rep. Geol. Surv. Pa., 1839; Geol. New Jersey, 1840, 166.

so wholesale a process, it is excluded first by the occurrence of coarse conglomerates on the eastern margin of the formation where they dip strongly toward the ledges of crystalline rocks from which they were doubtless derived, and from which they are now separated by a well-proved fault, with heavy downthrow on the western side; second by the numerous faults in the middle area of the formation, whereby the trap-ridges are so systematically repeated. Whatever hypothesis be accepted in explanation of the monoclinal attitude, its first requisite is for these reasons that the strata have actually been disturbed since their deposition.

It has been more recently proposed * to account for the Triassic monoclinal in New Jersey, which in so many ways resembles that of the Connecticut Valley, by a progressive disturbance during the deposit of the strata. The mechanical difficulties in the way of this process are to my mind rather formidable, but independently of these, it cannot be accepted for the Connecticut Valley, because it does not account for the occurrence of faults that here form so characteristic a part of the structure and that are so essentially connected with the rest of the deformation. Moreover, before the faulting the strata now disjointed must have been continuous; and the nearly uniform thickness over large areas of the trap overflows that are buried between the sedimentary beds shows that they at least could not then have been inclined at their present dip, but that the whole series must have lain essentially horizontal. Finally, the displacement of the highest as well as of the lowest members of the series by the faults, and probably in some cases by the same fault, indicates that these dislocations did not begin till after the whole formation was practically completed; or to put it in a more natural form, this indicates that the period of deposition continued until it was closed by the elevation accompanying the disturbances that have given rise to the existing structure. It is probable, on the other hand, that certain uneven changes of level took place during the accumulation of the strata, for they are of considerable thickness on a rather limited area, they present many marks of shallow water during their deposition, and extended eruptive action characterized the latter part of the period; but these changes may practically be neglected in seeking for an explanation of the existing structure. The second requisite of an acceptable hypothesis is therefore that it should recognize the original horizontal position of the beds, and place their disturbance after the accumulation ended.

The occurrence of igneous rocks so closely associated with the Triassic formation, not only in the Connecticut Valley but

* Cook, Geology of New Jersey, 1868, 174.

in its several other areas, where the monoclinal structure is also developed, early gave rise to the theory that the intrusion of these rocks must have caused the tilting of the adjoining beds. Whatever possibilities of disturbance such intrusions may possess, their action is certainly excluded from the greater part of the Connecticut Valley area, for here nearly all the igneous rocks are found to be contemporaneous overflows: the intrusive sheets form much the smaller share of the trap-ridges and seem to be limited to the western border of the formation. The overflows having taken their present position among the strata while the formation still lay horizontal, could have had no active share in disturbing it, but must instead have passively suffered deformation with it. It is very probable that the intrusive sheets also took their present position in the sedimentary series before the formation was tilted and faulted. Similar intrusive sheets are known among strata that still lie horizontal, and that were no more disturbed at the time of intrusion than was necessary to give place to the lavas that were driven between them. No peculiarity of structure is found in the neighborhood of these Triassic intrusions that is not found elsewhere in the formation; but on the contrary the structure that they and their adjoining sandstones present is a remarkably faithful imitation of the structure that is at once so peculiar and so characteristic in the overflow sheets in other parts of the formation; and even if the disturbance in the neighborhood of the intrusions be attributed to their action, this singular imitation remains to be accounted for. Indeed there is not only an imitation but a correlation in the structure of the districts occupied by the intrusions and overflows: the ridges formed on their outcropping edges are all convex to the west; where they are disjointed, their ends overlap or offset in a similar fashion; the overlaps or offsets of the West Rock range being in what Percival called the advancing order (south end of northern member overlapping north end of southern member on the west), like the Hanging Hills and other southern overflow ridges; while the Barn-door Hills in Granby, which seem to be the northern representative of the West Rock range, overlap in receding order, like the northern half of the great range that extends from the Hanging Hills to Mounts Tom and Holyoke. In view of this, it can hardly be doubted that the intrusive sheets as well as the overflows and bedded rocks once lay horizontal, and that the whole mass of strata, igneous as well as aqueous, was subsequently and passively deformed by an external disturbing force.

The idea that the whole monoclinal represents an unfaulted mass, tilted to an almost constant eastward dip,* has not gen-

*This is most definitely presented in LeConte's Elements of Geology, 1878, 441.

erally found acceptance on account of the great thickness of strata that it involves. It implies indeed a disturbance of improbable simplicity, and is directly excluded by the presence of numerous strike faults, always with upthrow on the east, whereby single beds are enabled to do more than double duty, and a not immoderate thickness suffices to account for the breadth of the formation. Many of the smaller faults are almost directly visible: the evidence on which the existence of the larger ones rests is of the same character as that which is accepted to prove the occurrence of faults in such a region as Tennessee; namely, the repetition of corresponding series of beds. It is true that some members of the series here are sheets of trap; but when these sheets are found to be contemporaneous overflows, they at once take rank as conformable members of the formation, and gain a high value as indications of structure from the success with which they resist erosion and the prominence that their outcrops therefore maintain. To deny the existence of the larger faults requires one to admit the frequent recurrence of a definite complex sequence of formative conditions repeatedly causing the deposition of a series consisting of sandstone, amygdaloidal trap, limestone, shales, heavy trap, sandy shales, lighter trap and sandy shales, in this regular succession and of closely corresponding thickness, member for member. This is so inherently improbable that it would not be admitted for a moment, if the trap-sheets were sedimentary rocks, like the ridge-making sandstones of Tennessee; and it is difficult to see how the eruptive origin of the sheets lessens the improbability of the supposition. To admit the existence of the faults allows a natural explanation of these repeated sequences, reduces the otherwise surprising thickness of the formation to a moderate measure, and simplifies the eruptive action of the period by correlating nearly all the trap-ridges as the repeated outcrops of a very few overflow-sheets. If the fault-lines thus indicated were disorderly, and could not be shown to be related to one another, very complete evidence might be demanded in proof of them; but they are most systematic in their position. In any single district, they are all nearly parallel to one another; south of Hartford, their trend is north-northeast or northeast; north of Hartford, they follow closer to the meridian. The upthrow is on the east, with insignificant exception. It does not seem illegitimate to regard so systematic an arrangement as a valuable corroboration of the evidence that first indicated the existence of the faults, and even to use it as a means of extending their discovery to localities where the evidence from sequences could not be fully applied for want of sufficient outcrops. It has in this way been made at least extremely probable that all the large trap

ridges from Talcott Mountain and the Hanging Hills to Toket and Pond Mountains near Long Island Sound are simply repeated outcrops of a single heavy sheet of trap, separated by faults; and that these faults and others belong to a system of fractures that divide the formation into a number of long, relatively narrow blocks, in every one of which the beds dip as a rule to the east at a moderate and tolerably constant angle.

The simple monoclinal also fails to explain the crescentic curvature of the trap ridges, that so early awakened interest. Their form cannot be attributed to the curvature of the fissures through which the trap was supplied, for the two finest examples, Pond and Toket Mountains are found to be overflow sheets, not intrusions; and the sedimentary beds below and above them are curved conformably with them. Among the observations that prove the contemporaneous origin of the trap, the following may be mentioned: the sandstone directly overlying the Toket Mountain trap-sheet contains many fragments of trap; and the cavities of its waterworn amygdaloidal surface are found neatly filled with fine, unbaked sediment. A very coarse conglomerate, containing numerous bowlders of trap, is closely associated with the posterior ridge of Pond Mountain. The two mountains must be regarded as the curved outcrops of gently folded or "dished" strata, cut off on the eastern side by faults. The crescentic form of the ridges is indeed closely correlated with the whole system of faults; for in several cases their curvature is increased by a succession of small dislocations on the hooks of the crescents. For example, the southern end of Pond Mountain, where crossed by the New Haven and Branford highroad and by the Shore Line Railroad, is notched by oblique faults, whose eastern upthrows materially add to the departure of the ridge from a straight line. Larger illustration of the same structure is seen in the dislocated members of the Hanging Hills by Meriden, and of the Mount Holyoke range in Massachusetts. Moreover, this correlation of faint folds with faults explains the order of overlays or offsets in the successive ridges of a curved range; at the southern end of a crescent, the upthrow on the eastern side of the faults produces the advancing order, seen in miniature at the southern end of Pond Mountain, and in a much larger way along the Hanging Hills and as far north as Talcott Mountain; at the northern end of a crescent, the same upthrow produces the opposite overlap, or receding order, as in the little ridges of the range that crosses the Mattabesick at Beckly station, or again in the peaks of the Holyoke range.

The topography of Pond and Toket Mountains (Percival's E, I and E, II) is particularly instructive when thus interpreted. Pond Mountain is the simpler of the two, and is espe-

cially interesting from the appearance of its posterior ridge in an almost complete oval, the only example in the whole valley where the outcrop of a trap-sheet on the eastern side of its flat, dish-like fold is not cut off by a fault. The escape here is by only a few hundred feet, for the crystalline rocks appear close by. Such an eastern outcrop explains the steep slope and convexity of the ridge to the eastward, instead of to the westward, as is elsewhere the universal rule; and it is with no small satisfaction that so pronounced an exception to the rule is found to be so completely in accord with the explanation proposed for the structure of the formation: the exceptional case might have been predicted before observation. Toket Mountain is not separated from Pond Mountain by a fault, but by erosion on a transverse flat arched anticlinal, clearly defined by the dip of the adjacent conformable sandstones. Toket itself is divided into two unequal lobes by a faint indentation north of its middle; and from an examination of the dip of the accompanying sandstones it becomes clear that this indentation results from a faintly developed transverse anticlinal, of the same kind as that which, when carried farther, has caused the separation of the Pond and Toket crescents in the present stage of erosion. Indeed, the fate of separation has already overtaken the ridge posterior to Toket, which is cut into two crescents just opposite the indentation between the two lobes of the main mountain.

1.

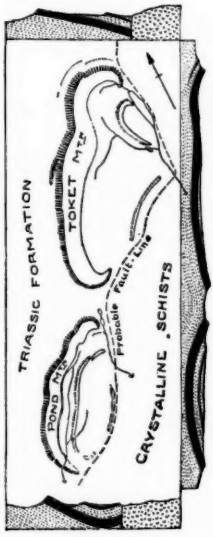


Fig. 1.—Pond and Toket Mountains, enlarged from Percival's outline. A transverse section is given for each mountain and a longitudinal section runs through both.

There is good reason to think that the wide separation of the Mount Holyoke and Deerfield trap ranges in Massachusetts is due to a similar transverse uplift, but of much greater value: for on this supposition we should expect to find the lowest members of the formation exposed about midway between the strong eastward curves of the ranges; and surely enough it is in precisely this position that the fundamental crystalline rocks of Mount Warner appear, and they have no other outcrop within the limits of the Triassic formation. The combination of faults and faint, dish-like folds in the way here outlined is therefore not only allowed but demanded by the topography; and there is good promise that the intricate maze of ridges

represented on Percival's map may be reduced to system when their many forms are interpreted as slight variations on one or two simple types of structure. The trap ridges of the little Woodbury-Southbury Triassic area in Western Connecticut present the same types of structure and much was learned from their examination. The explanation of the Triassic disturbance by external forces must for these reasons devise a mechanism for the production of a faulted monoclinal, in which the faults will have their upthrow of varying value on the side toward which the beds dip, and in which a more or less pronounced "dishing" of the beds may appear on a larger or smaller scale.

A few years ago, it was suggested* that certain disconnected Triassic areas with opposite monoclinal dips might be regarded as the buttress-like remnants of great anticlinal arches, the larger part of which had been consumed by erosion. From what has already been said, it is apparent that a simple, unfaulted anticlinal cannot be accepted, at least for the Connecticut buttress of the Connecticut-New Jersey arch; and the former continuity of the Triassic beds over the crown of the arch, so as to cover all the surface between the present limits of formation, is not only unproved by independent evidence, but is unnecessary to the hypothesis; for it is clear that if the foundation, on which the Triassic strata rested, were greatly elevated in the Western Connecticut region, the strata would be tilted in opposite directions on the two sides of the elevation, whether they ever stretched across it or not. But whatever was the former area of the Triassic deposits, the suggestion that their disturbance was physically connected, and that it involved the disturbance of a much larger area of the earth's crust than that which they now cover, is a point of fundamental importance. It has been too much neglected because the rocks about the Triassic areas are so generally crystalline schists and gneisses, whose disturbance is not easily recognized. The absence of tilted sedimentary rocks all over Southwestern New England cannot be taken as evidence that no post-Triassic tilting occurred there; on the other hand, the little Woodbury-Southbury Triassic area gives direct evidence—not of the former continuity of the Triassic strata from Connecticut to New Jersey—but of the continuity of the disturbing action all across this district. So wide a disturbance must have had a deep penetration; and the faults in the Triassic areas give sufficient evidence that the dislocations were not limited to the relatively superficial coating of Triassic strata. Some of the faults have a throw of several thousand feet—for example, the

* Independently by Kerr (1874) for North Carolina, and by Bradley (1876) and Russell for Connecticut and New Jersey.

one that separates Lamentation Mountain from the Hanging Hills—and if drawn on true scale it cannot be supposed that displacements of so great a measure could fade away within the thickness of the Triassic beds, even if they were ten or fifteen thousand feet thick. The faults must penetrate to a depth many times greater than their throw; to a depth commensurate with their length, and this may be five or ten miles. This gives us literally a deeper insight into the Triassic disturbance, and points out another important requisite in the hypothesis for its explanation: the disturbance must affect not only the Triassic rocks, but a broad and deep mass of the earth's crust with them. Attention has been too generally given to the Triassic rocks alone; it will be well to invert the problem, and look for a time at the underlying schists and gneisses to discover how they may have yielded to a disturbance; letting the superficial covering of Triassic beds take the consequences of it, and adapt themselves to the dislocation of the surface on which they rest as best they may.

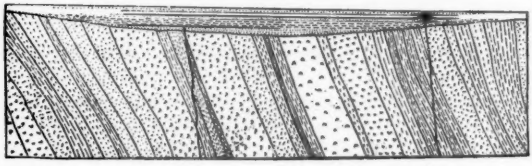
The fundamental rocks are great slabs of crystalline schists and gneisses, varying strongly in composition and texture. Some are massive and almost granitic; others are finely foliated. The attitude of the slabs is hardly more constant than their composition; but they are in general more nearly vertical than horizontal, and their prevailing strike is north or east of north. The Triassic strata were laid down on the edges of these steeply inclined slabs after they had been bevelled off to a tolerably even surface by pre-Triassic erosion. The character of the deformation of the schists would depend on the action of the deforming forces.

The general position of the Triassic areas as well as the detail of their structure at once suggests their connection with the Appalachian system of disturbances. The depression of the long trough-like areas in which the Triassic strata accumulated as well as the subsequent disturbance of the accumulations is most naturally referred to a late manifestation of the forces that had at an earlier date produced the great system of Appalachian folds; and as such should be regarded as the results of a slow, irresistible, horizontal crushing force or compression, acting east and west or southeast and northwest. No definite limit can be set to the depth through which the compression acted; but judging by its effects in the writhing and folding of the Appalachian strata, it must have had a profound penetration; and if the theories that have been suggested to account for the compression may be trusted at all as guides, the thickness of earth's crust affected by it must be measured by tens or even hundreds of miles.

A group of inclined slabs, compressed by a horizontal force

about at right angles to their strike, might yield in part by minute internal rearrangement, and in part by slipping on their divisional surfaces, so as to reduce their breadth by standing more nearly vertical, that is, more nearly at right angles to the compressing force. In so doing, the upper surface of the group would be somewhat elevated, and at the same time the bevelled edge of every slab would be tilted over by a tolerably constant angle in one direction, and separated from the neighboring slabs by a dislocation with the uplift on the side of the direction of dip. In case the compression varied at different depths, diminishing downwards, a shearing force would be introduced, by which the slabs could be thrown over past the vertical. It seems as if some such mechanism as this might account for the faulted monoclinal structure of the overlying Triassic strata, which must follow its hidden action.

2.



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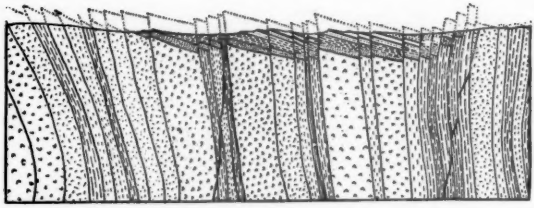


Figs. 2 and 3.—Hypothetical sections to illustrate the relation of the Triassic strata to the schists which they overlie unconformably. The dikes that supply the intrusive and overflow sheets of trap are drawn in assumed positions; the simplicity of the attitude of the schists is without doubt much exaggerated. When the breadth of the section in the first figure is reduced by a compression that reaches to a much greater depth than the sections include, the slabs of schist slip on one another so as to take more nearly vertical positions, and the overlying beds are thereby broken into blocks and canted over into a monoclinal attitude, with a dip equal to the change in the inclination of the schists; the faults between the blocks have their upthrow constantly on the east. It is not desired to affirm that the fault planes are in continuation of the foliation of the schists. The hade of the faults has not been determined.

It is difficult to consider all the natural possibilities of such a process. The number and closeness of the faults would depend on the texture of the schists: the more numerous and closer

the faults, the less would their throw generally be. A change in the number and throw of the faults along their strike would warp the upper surface into flat, dish-like corrugations.

According to this hypothesis, the fissures which divide the Triassic strata into long, relatively narrow blocks are to be regarded as determined by the divisional surfaces between the slabs of the underlying schists; as such, the direction of the Triassic faults should correspond with the strike of the underlying schists. This must be the explanation of the oblique, north-northeast direction of the topographic outlines from Hartford to the Sound: for Percival's map shows a broad belt of schists, with strike about north-northeast, approaching and running under the Triassic area at its southwestern margin near New Haven, directly towards the Hanging Hills, where the obliquity of the fractures between the trap ridges is so well seen; continuing nearly in the same direction, the eastern margin of the Triassic area is found bordered by schists with similar strike, from Glastonbury to Stafford.* The strike of the buried schists determines the strike of the surface structure and topography; and as the correspondence of these two apparently independent elements was first noticed after the reason for it came to mind, it seemed to me a valuable confirmation of the hypothesis here presented. Nearly all the ridges of this southern district overlap in advancing order, because the faults, with upthrow on the east, here trend a little to the right of the strike of the trap-sheets, with dip to the east. Where the overlap is receding, it is because the ridges have curved so far to the east that the faults run to the left of their strike, as in Berlin and southern Wethersfield, or on the northern hooks of the Pond and Toket crescents. There is also a necessary correlation between the throw of a fault and the amount of overlap and offset that it will produce: the fault between the Hanging Hills and Lamentation Mountain has probably the greatest throw of any yet found, and the greatest overlap and offset is found between the same ridges. The unique topographic details of the district thus find a simple explanation in the structure that results directly from a single process of mechanical disturbance.

The receding order of overlap in the range of the Barndoor Hills of Granby and Simsbury offered opportunity of making further test of the correspondence between the strike of the schists and the trend of the faults and the resulting displacement of the ridges. Between Simsbury and Canton, the range bears north and south; the underlying and neighboring schists should therefore run west of north, so that their strike shall be

*Percival noticed this alignment, but doubted the continuity of the schists across the Triassic depression. Geol. Conn., 222, 289, 290.

to the left of that of the ridges. Farther north in Granby, the range turns to the northeast, and here a north and south strike of the schists would produce a receding overlap. Having determined this from the conditions of the hypothesis, the mazes of Percival's text were searched through with care, and it was then found that the schists in Canton, close to the first-named ridges, dip to the east-northeast, and therefore strike to the north-northwest (Geol. Conn., p. 78); while in the second locality, the schists strike about north and south (Id., pp. 75, 293, 294). Further tests of this nature will be made by additional observation in the field.

There is a difficulty in the operation of the mechanical disturbance that should be referred to. The faults are sometimes so close together, that it is not easy to imagine them extending downward across the whole series of underlying beds without causing irregularities of dip; and yet no such irregularity is thus far discovered. But it may perhaps be that the present surface of the ground was so deeply buried during the initial stages of the disturbance, that the weight of the superincumbent mass, or gravitative form-destroying force, was then and there greatly in excess of the strength of the rocks, or cohesive, form-preserving force; in such case, the fractures would follow closely along the planes of greatest stress as indicated by the slabs of schists, and once formed they would long remain the surfaces of easiest displacement. However this may be, the hypothesis seems to accord well in its essential features with all observations yet collected, and to possess something of the prophetic power that may in time entitle it to be called a theory. It may perhaps find application in other regions where unconformable masses have been deformed together, and the lesser, superficial mass has to accommodate itself to the forms presented to it by the greater mass beneath. When compression overtakes the region of northern France, where the massive and steeply inclined coal-measures are covered unconformably by a relatively thin sheet of Cretaceous strata, the latter may be broken up into a number of tilted blocks, not unlike our Triassic. It may be that even so great a faulted monoclinal as that of eastern Tennessee owes a good part of its structure to the reaction from its crystalline foundation.

Art. XL.—*Researches on the Lithia Micas;** by F. W. Clarke.

I. The Lepidolites of Maine.

In the western part of Maine, along a line running southeasterly from the Rangeley Lakes to a point on the seaboard between Brunswick and Portland, there are a series of veins of albitic granite which are noted for their contents in lithia mica and colored tourmalines. These localities, in the towns of Rumford, Paris, Norway, Hebron and Auburn, are all within a narrow belt of about forty miles in length; and with them, as a probable part of the same system, may be classed the spodumene locality in the town of Peru. The northernmost of the known localities is that on Black Mountain in Rumford; but a few fragments of inferior green tourmaline have been found about five miles farther north, in Roxbury—a fact which indicates a prolongation of the belt in that direction. Similarly, a southern extension of the belt is suggested in the territory covered by the towns of Pownal, Yarmouth and Freeport, a region from which a few casual specimens of lepidolite have been reported. The total width of the belt, so far as has been observed, appears to be not much over fifteen miles, although sharp definition of the area is not yet possible.

In general character the several localities are much alike, although in points of minor detail they vary considerably. With the tourmaline and lepidolite, quartz, muscovite, cleavelandite, cassiterite and amblygonite are always found, together with a wide range of other species which cannot be considered here. Some of the differences are probably due to the fact that certain localities have been more thoroughly opened up than others, and these differences would probably be eliminated by more complete exploration. In other points, however, the variations are notable and characteristic. For convenience we may briefly consider the several localities separately, beginning with the northernmost and proceeding southward in regular order.

Rumford.†—This locality, discovered in 1878 by Mr. E. M. Bailey, of Andover, Maine, is situated on the northern slope of Black Mountain, at an estimated elevation of about one thousand feet above the valley. The excavation, so far, has been merely superficial. A part of the lepidolite, which is very abundant, is fairly pure, lilac-purple in color and finely granular; but the larger portion of the mineral is coarser in structure and thickly sprinkled with small, opaque, red tourmalines.

*Abstract of a paper to appear in full in a forthcoming Bulletin of the U. S. Geological Survey.

† See G. F. Kunz, Proc. Amer. Assoc., 1885.

The color of the latter is very rich and the appearance of the material carrying the associated minerals is strikingly characteristic. It resembles nothing from the other localities of the region. Green tourmaline occurs here but sparingly, while the spodumene is enormously developed, showing faces over a metre in length. At the other lepidolite localities spodumene is one of the scarcer minerals.

Paris.—The famous locality known as Mt. Mica has been so thoroughly studied by mineralogists that no detailed account of it is needed here. The lepidolite occurs both in the ordinary, purple, granular form and in broad foliations resembling muscovite. Analyses of it have been published by Berwerth and by Rammelsberg,* which agree fairly well with the results obtained in this laboratory.

Hebron.—The deposit at this point, about seven miles southeast of Mt. Mica, has long been well known to collectors. It has only been superficially opened, however, and deserves more systematic exploration. The lepidolite, which is coarsely granular and purple, is especially interesting on account of its yield in cæsia and rubidia, which, after their identification by Johnson and Allen here, were also found in the lepidolite of Rozena. As yet no complete analysis of the Hebron mineral seems to have been published. For the specimens examined in this laboratory I am indebted to the kindness of Professor G. J. Brush.

Associated with the Hebron lepidolite there have been found many specimens of red and green tourmaline, which, while preserving their crystalline form, have undergone an alteration into a softer mineral of an opaque, talcose appearance. Some of the material so derived from tourmaline has been supposed to be lepidolite; and as it was possible that a study of it might have interest, an analysis was made by Mr. R. B. Riggs. The specimen chosen was originally rubellite, and its specific gravity, as determined by Mr. T. M. Chatard, was 2·87. Analysis as follows:

SiO_2	43·90
Al_2O_3	38·71
Fe_2O_3	·58
FeO	·25
MnO	·04
CaO	.41
MgO	·05
Na_2O	1·05
K_2O	10·92
H_2O	4·25
F	none
B_2O_3	trace
	100·16

* See third supplement to Dana's Mineralogy, pp. 78, 79.

These results show clearly that the alteration product is not lepidolite but damourite, a fact which could hardly be altogether unexpected. The material analyzed was received from Mr. N. H. Perry of South Paris, Maine.

Norway.—The specimens from this locality were partly collected during a field expedition, and partly received from N. H. Perry. The lepidolite was of two varieties; one, pale brown and very finely granular, the other much coarser in structure and white. Most of the associated tourmaline has a peculiar, dark oily-green tint; and with many of the specimens a rose red earthy clay was associated. A partial analysis of the latter was published in Bulletin No. 9 of the U. S. Geological Survey, but a re-analysis seemed to be desirable and was therefore made by Mr. Riggs. The results are subjoined and show that the mineral approaches nearly to cimolite. The difference from the earlier analysis indicates a non-homogeneity of composition. As to the genesis of the clay, nothing can be said with certainty.

SiO_2	66·86
Al_2O_3	22·23
Fe_2O_3	·47
FeO	·18
MnO	·07
CaO	·42
MgO	·33
Li_2O	·29
Na_2O	·46
K_2O	·18
H_2O	8·26
F	·06
	99·81

Formula, $Al_2H_4Si_2O_{10}$.

Auburn.—In the western part of this town, near the Minot line, there are two localities for lepidolite, less than half a mile apart. The one longest known is small and unimportant, but the other on the farm of G. C. Hatch, has been quite thoroughly developed.* It has yielded many gem tourmalines, mostly of the paler tints, some remarkable apatites, and perhaps the best crystallizations of lepidolite so far known. The latter mineral occurs in the ordinary purplish, coarsely granular form and also in extraordinary perfection as a border upon muscovite; the broad plates of the latter being practically encircled by aggregations of small crystals of the lithia mica. Some specimens of this type have also been found at Paris.

As it was hoped that this mode of occurrence of lepidolite might throw some light upon its genesis, three analyses of Auburn material were made by Mr. Riggs: first, of the com-

* See G. F. Kunz, this Journal, III, xxvii, 303.

mon granular variety, secondly of the border upon muscovite, and finally of the latter mineral from the center of the second specimen. The muscovite analysis came out as follows:

SiO_2	44·48	Na_2O	2·41
Al_2O_3	35·70	K_2O	9·77
Fe_2O_3	1·09	H_2O	5·50
FeO	1·07	F	·72
MnO	trace		100·84
CaO	·10	Less oxygen	·30
MgO	trace		100·54
Li_2O	trace		

About these results there is nothing unusual.

For convenience the analyses of lepidolite, all by Mr. Riggs, may be given in one table. The water in every case was determined directly by means of the Gooch tubulated crucible, and most of the lithia separations were effected by a new process, lately devised in this laboratory by Dr. F. A. Gooch, and shortly to be published in full. In the Auburn and Norway lepidolites, however, the old phosphate method was used. The fluorine estimations were all by the Berzelian process. In the figures given for $(KRbCs)_2O$, we have actual determinations based upon the amount of chlorine in the mixed chlorides, and not a mere computation as K_2O from the weight of the chloroplatinates. All important estimations were checked in duplicate, and the table of results represents mean values. Seven analyses were made, as follows:

Analyses of Lepidolite.

	A. Purple. Rumford.	B. Foliated. Paris.	C. Granular. Hebron.	D. Border. Auburn.	E. Granular. Auburn.	F. White. Norway.	G. Brown. Norway.
SiO_2	51·52	50·92	48·80	49·62	51·11	49·52	50·17
Al_2O_3	25·96	24·99	28·30	27·30	25·26	28·80	25·40
Fe_2O_3	·31	·30	·29	·31	·20	·40	.87
FeO	undet	·23	·09	·07	·07	·24	·45
MnO	·20	trace	·08	·55	·17	·07	·23
CaO	·16	trace	·10	----	·12	·13	undet
MgO	·02	trace	·07	----	·01	·02	undet
Li_2O	4 90	4·20	4·49	4·34	4·98	3·87	4·03
Na_2O	1·06	2·11	·74	2·17	1·43	·13	13.40
$(KRbCs)_2O$	11·01	11·38	12·21	11·19	12·25	12·63	
H_2O	·95	1·96	1·73	1·52	·94	1·72	2·02
F	5·80	6·29	4·96	5·45	6·57	5·18	5·05
	101·89	102·38	101·86	102·52	103·11	102·71	101·62
Less oxygen	2·44	2·64	2·02	2·29	2·76	2·18	2·13
	99·45	99·74	99·84	100·23	100·35	100·53	99·49

As regards cæsium and rubidium the Maine lepidolites offer some points of difference. In the Rumford material an ordinary analysis shows neither; but a special examination upon

the alkalies from 150 grams of mineral gave a spectroscopic trace of rubidium. The Paris lepidolite showed traces of both metals, while the material from Hebron, Norway and Auburn was much richer. On these lepidolites approximate estimations were made, as follows:

	Hebron. Granular.	Norway. White.	Auburn. Granular.	Auburn. Border.
K_2O	11·44	8·82	10·51	8·03
Rb_2O }	·77	3·73	1·29	2·44
Cs_2O }		·08	·45	·72
	12·21	12·63	12·25	11·19

In these determinations the cæsium was separated as stannic chloride, and the other two metals were computed indirectly from the amount of chlorine in the mixed chlorides. The results have only a comparative value, and no sharp accuracy can be imputed to them. The methods of separation for the three metals are still far too defective.

The foregoing analyses of the Maine lepidolites, covering several distinct types of the mineral from five different localities, indicate a great constancy of composition. The essential identity of the mineral with that from Rozena and Cornwall is also shown by the several published analyses of the latter, while the lepidolite from Juschakova is slightly different, in that it contains some manganese replacing aluminum, higher fluorine and a little chlorine. In most of the analyses the water and fluorine appear to vary reciprocally, suggesting the ordinary replacement of the latter element by hydroxyl. With this slight assumption the formula for lepidolite may be written thus:

$$Al_2LiKF_2Si_3O_9;$$

a formula which has long had general acceptance, but which now rests upon the surer basis of a wider range of analytical data. It corresponds to the following theoretical composition:

	Calculated.		Found.		(Riggs).
SiO_2	49·18		48·80	to	51·52
Al_2O_3	27·87		24·99		28·80
Li_2O	4·09		3·87		4·98
K_2O	12·81	$(NaKRbCs)_2O$	12·07		13·68
F	9·84	(FH_2O)	6·69		8·25

Most of the variations are no greater than we should expect to find in material so difficult to secure in absolute purity as lepidolite. The granular structure of that species is peculiarly favorable to the presence of inclusions, as for example of albite, to which probable impurity some of the soda shown by the analyses may be due. The greatest difference is in the case of fluorine; although some of the foreign analyses, notably Rammelsberg's analysis of the Juschakova mineral, contain very nearly the full theoretical amount.

II. The Iron-Lithia Micas of Cape Ann.

In the granite quarries of Rockport, Massachusetts, near the extremity of Cape Ann, there are occasional feldspathic veins which contain the rarer minerals danalite, cyrtolite, fergusonite, and amazon-stone, together with certain remarkable micas. One of the latter, cryophyllite, was described by Cooke in 1867;* who also analyzed an associated "lepidomelane," to which Dana afterward gave the name of annite. The vein from which Cooke obtained his material was long ago blasted away or covered up; but other veins of like nature are still accessible, and from one of them the micas examined in this laboratory were obtained. They were collected by the original discoverer of the locality, Mr. W. J. Knowlton of Boston, and were, to all appearance, identical with the micas described by Cooke. Among the specimens the two types were clearly recognizable; the one a dark-greenish-black lithia mica, cryophyllite; and the other a black, brilliant lepidomelane. In some specimens the cryophyllite formed a border upon the broad plates of the other mica, precisely as in the association of lepidolite and muscovite at Auburn. The resemblance in this particular was curiously striking; only the Rockport minerals are less conspicuous than those from Maine, since they lack the contrast of color which gives the latter their beauty.

The cryophyllite varies considerably in outward character, and three well marked types of it were analyzed by Mr. Riggs. They may be described as follows: A. Broadly-foliated, brilliant, blackish-green. B. Paler, dull-green, less lustrous, seemingly altered. C. An aggregation of minute six-sided prisms, dark green, almost granular in appearance, resembling some chlorite. Mean results are given.

	A.	B	C.	Cooke's Analysis.	
SiO_2	51·96	51·46	52·17		51·49
Al_2O_3	16·89	16·22	16·39		16·77
Fe_2O_3	2·63	2·21	4·11		1·97
FeO	6·32	7·63	5·99		7·98
MnO	·24	·06	·32	Mn_2O_3	·34
CaO	·12	trace	trace		----
MgO	·03	·17	trace		·76
Li_2O	4·87	4·81	4·99		4·06
Na_2O	·87	·89	·63		trace
K_2O	10·70	10·65	10·48		13·15
H_2O	1·31	1·12	1·46		----
F	6·78	7·44	7.02	SiF_4	3·42
	102·72	102·66	103·56		99·94
Less oxygen,	2·86	3·11	2·95		
	99·86	99·55	100·61		

* This Journal, II, xliii, 217.

It will at once be seen that the three samples examined by Mr. Riggs were sensibly identical, and not far in composition from the material analyzed by Cooke. In the latter the fluorine amounted to 2·49 per cent, which is only about one-third of the figure found in this laboratory, while the silica, plus that represented in the SiF_4, is sensibly higher. The integrity of the species, however, seems to be clear; and it cannot be identified with zinnwaldite. Empirically, as deduced from the new analyses, the formula of cryophyllite may be written

$$FeH_3K_3Li_4Al_4F_4(SiO_3)_{10}; \quad \text{or,} \quad R'_4Al_2F_2(SiO_4)_3.$$

With the "lepidomelane" or annite, quite unexpected results were obtained. The material analyzed by Mr. Riggs was black, brilliant, broadly foliated, and apparently very pure. His figures are given in contrast with those published by Cooke.

	Riggs.		Cooke.
SiO_2	31·96		39·55
TiO_2	3·42		----
Al_2O_3	11·93		16·73
Fe_2O_3	8·06		12·07
FeO	30·35		17·48
MnO	·21	Mn_2O_3	·60
CaO	·23		----
MgO	·05		·62
Li_2O	trace		·59
Na_2O	1·54		trace
K_2O	8·46		10·66
H_2O	4·25		1·50
F	trace	SiF_4	·62
	100·46		100·42

It is at once evident that two entirely distinct micas are here represented, and the question is raised whether the Rockport granites may not contain a series of complex isomorphous mixtures. Cooke, indeed, pointed out the isomorphism of cryophyllite with his samples of annite, and showed that the lithia and fluorine in the latter were probably due to admixtures of the former. We now see that at least three micas are involved in the problem to be solved, and the difficulty of establishing definite formulæ is enormously increased. For the present, approximate formulæ only can be assigned to these micas, involving several assumptions, and representing probabilities rather than complete interpretations of the facts. If we unite the groups SiO_2 and TiO_2 in the new annite, and regard the ferric iron as belonging partly with the alumina, and partly as having been derived by oxidation from the ferrous state, we have the following general formulæ for the two analyses.

Annite, Cooke, $R'_8Al_4(SiO_4)_6$.
Annite, Riggs, $R'_{14}Al_2(SiO_4)_6$.

For the former, the equivalent of R'_8 is approximately $K_2H_2Fe''_2$, and for the latter it is $K_2H_4Fe_4$. These values correspond to the following percentage compositions.

	Cooke.	Riggs.
SiO_2	39·5	36.6
Al_2O_3	26·8	12·4
FeO	18·9	35·1
K_2O	12·4	11·5
H_2O	2·4	4·4
	100·00	100·00

Attention has already been called to the fact that some specimens of cryophyllite form borders upon plates of annite, precisely as the lepidolite of Auburn is arranged about nuclei of muscovite. It accordingly becomes quite probable that similar relations connect the two pairs of minerals; and upon these relations the formulæ so far deduced shed some light. In each case we have a mineral with metasilicate ratios implanted upon an orthosilicate; and a derivability of the one from the other is forcibly suggested. Structural analogies also appear; for in each couplet we have evidence of a common type of nucleus, which may be represented thus:

Lepidolite nucleus, $Al(SiO_3)_3$. Cryophyllite nucleus, $Al_2(SiO_3)_6$.
Muscovite nucleus, $Al(SiO_4)_3$. Annite nucleus, $Al_2(SiO_4)_6$.

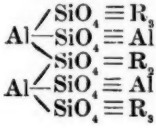
The development of complete structural formulæ from these nuclei is rendered difficult by our ignorance of the part which fluorine plays in such compounds. In the ordinary lepidolites it is easy to conceive of it as united with the aluminum in the univalent group AlF_2; but in the iron micas and phlogopite a similar representation is unsatisfactory. A different solution of the problem is therefore to be sought, and it may be found by an application of the well-recognized principle that fluorine and hydroxyl can replace each other isomorphously.

If now we start from orthosilicic acid, $Si(OH)_4$, and regard the hydroxyl groups as successively replaceable by atoms of fluorine, we can conceive of a series of acids ranging from $Si(OH)_4$ to SiF_4; and with the aid of such a supposition many of the fluoriferous silicates may be rationally explained. For example, the acid $SiF(OH)_3$ may be considered, and its nucleus, $SiFO_3$, a trivalent residue, can be applied to the discussion of the lithia micas. Upon this basis the formulæ for lepidolite and cryophyllite become curiously significant, especially when written in direct comparison with the structures assignable to muscovite and annite respectively. In every case the assump-

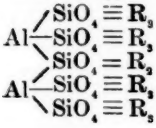
tion is made that double salts are to be represented as substitution derivatives of normal salts of similar type, and the results appear as follows:

Muscovite.

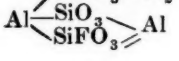
Annite (Cooke).



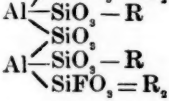
Annite (Riggs).



Lepidolite.

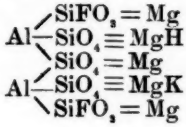


Cryophyllite.



To zinnwaldite, as represented by the published analyses, a probable structure is not easily assignable; and the variations in its composition suggest that the specimens examined have been mixtures of isomorphous compounds. It needs to be thoroughly reinvestigated, not only for itself, but also in its relations to whatever other micas may be associated with it. The matter of association can never, in the study of the micas, be safely neglected.

Although the formulæ herein assigned to annite and cryophyllite are to be regarded merely as trial hypotheses, there can be little doubt that the ratios between the sesquioxides and the silica are correctly given. These ratios are the ratios indicated by the published analyses of phlogopite; with which, therefore, rather than with ordinary lepidomelane, the Rockport micas are chemically to be classed. Zinnwaldite is already so classed by Tschermak and others, and cryophyllite falls easily into the same category. The subjoined formula fairly represents a typical phlogopite.



ART. XLI.—*The thickness of the Ice in Northeastern Pennsylvania during the Glacial Epoch;* by JOHN C. BRANNER.

THE thickness of the ice in northeastern Pennsylvania during the Glacial epoch was supposed, from such evidences as those who have studied the region were able to find, to have been indicated by the absence of striæ, bowlders or ice-worn fragments above an elevation of 2,200 feet above tide, upon the sides of the highest hills in that part of the State. These high points are Sugar Loaf peak, Ararat peak and Elk Mountain. The first two are the straggling continuation of the Lackawanna coal basin beyond where the Moosic range and the Lackawanna mountains coalesce. They are in Wayne County, almost upon its boundary with Susquehanna County, and about sixteen miles north and a little east of Carbondale. The summit of Sugar Loaf is 2,450 feet, and of Ararat peak 2,600 feet above tide, according to Professor I. C. White, who has reported upon this region.* Elk Mountain is in Susquehanna County, about ten miles north and a little west of Carbondale. It culminates in a pair of peaks known as North and South Knobs, which are respectively 2,700 and 2,575 feet above tide according to Professor White's measurements.

These several peaks are the highest points in the northeastern part of the State, and stand up boldly above the surrounding country, the valleys being eroded away about them to a depth of from 500 to 1,500 feet.

Professor I. C. White refers in several places to the absence of evidence that these peaks were covered by the ice, and they are spoken of, both by himself and by Professor H. C. Lewis, as having stood out like islands above the ice of the glacial epoch. Speaking of Ararat and Sugar Loaf, Professor White says:† "These summits, which probably existed as elevated hills in preglacial times, were left as islands in the ice moving southward, and escaped the universal abrasion to which every other portion of the township was subjected; for no appearance of drift or glacial scratches can be found on their sides or summits." In another place (ibid., p. 25) he speaks of "such isolated peaks as Elk Mountain, Ararat, and Sugar Loaf—the summits of which were islands in the *Mer de Glace*, and thus escaped its action." On the same page he says: "2,200 feet above tide is the greatest elevation at which I have observed direct evidence of glaciation, either in the shape of morainic *débris* or striated rock surfaces. All higher summits which I

* Report G5 of the Second Geological Survey of Pennsylvania.
† Report G5, Second Geol. Survey of Pa., pp. 158, 159.

have examined are destitute of drift deposits." Professor H. C. Lewis also refers* to these peaks as having stood up as islands in the sea of ice. He refers† to the evidence of ice action on the summit of Penobscot Knob, in Luzern County, at an elevation of 2,220 feet above tide, only twenty feet higher than the limit mentioned by Professor White. These, then, are the highest points in the State of Pennsylvania at which unquestionable evidences of ice action have been observed and recorded, namely: at 2,200 feet by Professor White upon the sides of the high peaks, and at 2,220 by Professor Lewis upon Penobscot Knob.

Having given attention to the glaciation of the region under consideration,‡ I was deeply interested, upon visiting this part of the State in July of the present year, to see and examine for myself the evidences of the thickness of the ice upon the sides of Elk Mountain, said to be the highest point in northern Pennsylvania.

Ascending this mountain by the path leading to the south knob, I found upon the ledge of rocks forming the principal outlook over the surrounding country, well defined glacial striæ. Those upon the east side of the ledge bear S. 60°–65° W., while those upon the west side bear S. 70°–72° W., magnetic. Professor White makes the elevation of this point 2,400 feet above tide. This observation alone therefore indicates that the ice was thicker than Professor White supposed.

Ascending to the summit of the south knob, I found nothing along the brushy sides of the peak to indicate ice action. On the summit itself, now covered with woods, the decay of the rocks has left nothing, as far as I could see by hastily made observations, to show whether or not the peak was ever covered by the ice. In the hope of finding a more resisting rock, and consequently something more conclusive, upon the higher north knob, in company with Professor Dudley, of Cornell University, Mr. D. N. Green, of Scranton, and with several of the younger members of my class in geology, I visited that part of the mountain which lies a mile farther to the north.

As the whole of the upper part of these peaks is covered with a thick undergrowth of bushes and briars, it is not possible, while the plants are in full foliage at least, to get any idea of the arrangement of loose material about the sides or summit, or of the best places to see the rocks in position. My observations, therefore, were confined to such fragments and outcrops as we happened upon in passing from one peak to the other.

* Report Z of the Second Geol. Survey of Penn., pp. 14, 15.

† Report Z, p. 106.

‡ "Glaciation of the Wyoming and Lackawanna Valleys," in Proc. American Philosophical Society, 1886, No. 123, pp. 337–357; and Glaciation of the Lackawanna Valley, in Proc. A. A. A. S., vol. xxxiv, p. 212.

In the depression between the two, at an elevation of about 2,400 feet, I found the rocks presenting the general features of ice-worn surfaces; but inasmuch as there were no striæ preserved, and as the original surface of the not very hard Catskill sandstone had generally disintegrated, it could not be said with certainty that the form of the rock had been produced by glaciation. Many large fragments, and some of the sandstone seen in place before reaching the summit of the north knob, seemed to indicate ice wearing. But in no case were these evidences beyond some question, owing always to the original surfaces having been degraded by weathering. From several of the ledges we turned back the thin soil, but the laminations of the decaying rock always came away with the turf.

Upon reaching the top I was at first greatly disappointed in finding no exposures of the rock in place. The summit area is a small flat space of less than half an acre of ground, most of which has been cleared of its brush by a triangulating party which has used this peak as one of its stations. Mr. Green and I, in the hope of finding some exposed rocks in place on the side where ice marks are most liable to be found, passed over the summit and a short distance down the north side, and then skirted around the peak toward the east. At an elevation of 2,530 feet I found what seemed to be evidences of ice action upon a rock immediately overlying the Catskill red, shaly sandstone. At 2,550 feet I found another fragment worn flat, evidently by ice, but retaining no well-defined striæ. Climbing up again toward the top of the peak along the eastern flank, I found that the crest of the knob was bordered on this side by a perpendicular ledge of rock, some ten or fifteen feet high, about the base of which were strewn many large, angular fragments. Climbing this ledge to the table-topped summit again, I was struck by the general flatness of this point and by the somewhat rounded appearance of the northern margin of this ledge. A narrow strip of the rock, from one to three feet wide, was exposed along the verge of the ledge. This bare space is being gradually encroached upon and covered by the moss and thin turf of the summit. The whole appearance of the rocks along the moss-covered edge was so suggestive of ice wearing that I examined all the exposed surfaces with the greatest care. My search was finally rewarded by finding positive and unquestionable evidence of glacial action. The original glaciated surface had been preserved on this exposed rock in one place over an area of three or four square inches, and this surface was ice-worn, polished and striated; all about it was more or less weathered. Turning back the mossy soil a couple of feet, we laid bare a number of striæ a foot or two in length and as well defined as any I have ever

seen. They bear S. 34° W., magnetic. The point at which these striæ were observed is about fifty feet southeast of the triangulation station on the summit of the north knob of Elk Mountain. The elevation of this knob, according to Professor I. C. White, is 2,700 feet above tide.*

It has been supposed hitherto that we could measure the thickness of the ice in this part of the country upon the sides of these high peaks. The finding of well-defined striæ upon the very summit of the highest of them leaves us without a measure for the thickness of the ice in northeastern Pennsylvania. Other high points, such as Ararat, 2,600 feet, and Sugar Loaf, 2,450 feet above tide (White), only seven miles away and lower than Elk Mountain, must, almost necessarily, have been buried beneath the ice, although Professor White found no evidences of ice action upon their summits. The whole range of the Lackawanna Mountains northeast of the Susquehanna gap at Pittston has been examined by the writer. Evidences of glaciation were found upon its highest points along its entire length, or as far at least as Stillwater, above Forest City, where the Lackawanna River cuts through the range. The top of Bald Mountain, perhaps the highest point in this part of the range, and about 2,200 feet above tide, is striated in many places, while many bowlders, which seem to have been pushed up the northwest face of the mountain from the ledge not far below, are scattered over its summit. The striæ upon this eminence bear S., S. 6°, 10°, 11°, 20°, 25°, 33° W.

All my observations along the summit of the Lackawanna Mountains, and upon the high points here referred to, go to show that the great body of ice- and water-worn material ends long before the highest elevations are reached. In ascending Elk Mountain I did not see a single water-worn fragment above about 2,100 or 2,200 feet, though it would be extremely rash, of course, to say that such fragments do not exist above this elevation. This observation agrees entirely with those of Professor White.

It cannot be said that the disappearance of what was supposed to be the measure for the thickness of the ice in northeastern Pennsylvania, upon the sides of Elk Mountain, was altogether unexpected. The study of the topography and glaciation of the Lackawanna and Wyoming Valleys had already

* Report G5, Second Geol. Survey of Pa., p. 136. I have used the elevations given by Professor White for these summits, because he checked and re-checked them from Herrick Center, four miles away. When not otherwise specified, the measurements mentioned in this paper have been furnished me by Mr. D. N. Green, of Scranton, who used the railway elevations at Carbondale or Scranton as a base. I believe his elevations have not been checked. He does not make the elevation of the north knob of Elk Mountain as great as Professor White by sixty feet.

led me to believe that it would have been impossible for a sheet of ice 1,500 feet or less in thickness to flow across this mountainous region so utterly regardless, as the great glacier was, of its marked topographical features.

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ART. XLII.—*On the time of contact between the Hammer and String in a Piano;** by CHARLES K. WEAD.

HELMHOLTZ has, as is well known, developed a theory of the action of the hammer on a piano string. If the two remain in contact for a time equal to $\frac{3}{14}$ of the period of vibration of the string, the relative strength of the overtones as computed seems to be about the same as he found in a piano between the limits $C_1=33$ d. v. and $c'=264$ d. v. But in the theory there are several assumptions, and it cannot be confirmed by judgments of the ear alone; for we do not yet know certainly, even if Fechner's law be true, what is the proper measure of the stimulus of a sensation; is it the energy of vibration as Helmholtz assumes, or the square root of it as others have tried to prove, or some less simple function? Further, how is the loudness of a sound, that is, the strength of the sensation, dependent on the pitch of the sound, when several sounds have the same intensity in mechanical measure? Lastly, have we any proof that the relative strength of the component vibrations in the air-wave is the same as in the string? Although the quality of the sound may remain unchanged as the vibration of the string dies away, we have, I believe, absolutely no proof that the efficiency of the sounding-board, bridge, etc., is the same for all tones of the scale or for all the overtones of a given string.

It has therefore for a long time seemed to me desirable to measure directly the time of contact—an experiment far easier than the settlement of any one of the doubtful questions just referred to. The results of some preliminary measures are, I think, of sufficient interest and accuracy to justify their presentation.

The method used is quite simple. An electric circuit was completed through a cell, a resistance-box, a galvanometer, a fine wire around the stem of the hammer, a slip of gold-foil (about $\frac{1}{2000}$ inch thick) glued to the face of the hammer, and lastly, the piano wire and frame. The contact between the hammer and string produces a momentary closing of the circuit and a throw of the galvanometer needle, from the

* Presented before the Am. Assoc. Adv. Science, at the Buffalo meeting.

amount of which the time of closing may be calculated. The minimum resistance of the circuit when closed by holding up the hammer was 2·7 to 3·0 ohms, to which a 10-ohm coil was added to diminish the uncertainty arising from any variable battery- or contact-resistance and to reduce the deflection. A telescope and scale were used at a distance of 1·6 meters.

The piano used was a Knabe Grand, put at my disposal by my former colleague, Professor C. B. Cady, who believes that such studies as these may throw light on the technically important subject of "touch." The *b* (just below middle *c*) was found on comparison with a Koeing's fork to make 255·4 d. v.; therefore $c'=270{\cdot}6$ d. v.

From the theory of the ballistic galvanometer we get the equation

$$t=\frac{\mathrm{T}}{\pi}\times\frac{\mathrm{R}\vartheta}{\mathrm{R}'\vartheta'}.$$

t = time in seconds of momentary closing of circuit.
T = time in seconds of single vibration of galvanometer needle.
R = resistance of circuit.
R′ = resistance of circuit when a constant deflection was observed.
ϑ = throw of needle, in scale divisions.
ϑ' = constant deflection, in scale divisions, with R′ ohms in circuit.
T = 5·87 sec. R = 13 ohms (12·7 for exps. 1 and 2).
$\mathrm{R}'\vartheta'=440000$.
$\therefore t=0{\cdot}000055\ \vartheta$ seconds.

The correction for damping is so small that we may neglect it—about 2 per cent. The results of the observations are shown in the accompanying table.

No.	Tone.	No. vibr. n.	No. obs.	Mean. ϑ.	t. Sec.	$t\times n$.	Stroke.
1	C_4	34	11	92·0	·0051	0·17	
2			7	113·0	·0062	0·21	Soft.
3			3	94·0	·0052	0·18	
4			2	82·5	·0045	0·15	Hard.
5			1	102·0	·0056	0·19	Soft.
7			1	83·0	·0045	0·15	
8			1	108·0	·0060	0·20	Soft.
16			1	94·5	·0052	0·18	
6	C	67·7	3	52·5	·0029	0·20	
12	B	127·0	2	34·0	·0019	0·24	
12			2	38·0	·0021	0·27	Hard.
11	c	135·3	6	35·0	·00193	0·26	
10	c'	270·6	9	16·0	·00088	0·24	
	c''	-----	--	-----	-----	----	Broken.
13	c'''	108·2	3	28·0	·00154	1·68	
14			3	20·5	·00115	1·23	
15			2	18·5	·00102	1·11	

The observations for c''' are explained by the fact that the gold foil stuck to the wires and so increased the time of contact abnormally, but the time decreased as the foil was torn away. In all cases there was a slight click (much most easily noticed in the higher notes) showing that the addition of the foil, thin as it was, changed the quality somewhat.

The observations on $C_,$ were quite satisfactory and establish the unexpected fact that the contact-time for a very soft stroke is about 20 per cent greater than for an ordinary or hard blow (the very few contradictory observations on higher notes are not sufficient to disprove this conclusion); for an ordinary blow on this string the time is just $\frac{1}{6}$ of the period instead of ·21 as Helmholtz estimated.

Note on Collision Experiments.

While the apparatus used in the above experiments on the piano was set up, a measure was made of the time of collision between two ivory balls such as are used in the ordinary class-room experiments. Several experimenters have published their results for metallic balls, but I have seen none for ivory balls; and as such observations are fitted to do something toward clearing up, in the student's mind, the rather hazy subject of impulsive forces and their measurement, I add my results.

The constants for determining the time were the same as above except that R=15 ohms.

$\vartheta = 20$ div.

$\therefore t = ·00129$ sec.

The diam. of the ball $= d = 3·9$ cm.

Its mass $= m = 55$ gms.

The radius of the arc over which the balls swung $= 67·5$ cm.

The chord of the arc $= 20·5$ cm.

$\therefore$ Final velocity $= v = 78·2$ cm. per sec.

$m\,v = 4301$ C. G. S. units.

Average force during contact $= mv \div t = 3276000$ dynes $= 3340$ gms.

Maximum force $= 6680$ gms. approximately.

Compression of ball $= s = \frac{1}{2}vt = ·05$ cm. approximately.

Diameter of circle of contact $= 2\sqrt{d\,s} = ·89$ cm. approximately.

Malone, N. Y., Sept., 1886.

ART. LXIII.—*Photographic Determinations of Stellar Positions;** by B. A. GOULD.

IT has been suggested that a short account of my work upon stellar photographs for the attainment of accurate observations might be acceptable to the astronomical section. My intention had been to attend this meeting as a listener and learner only; but I comply with the suggestion the more readily, since by a notable coincidence, I spoke upon the same subject in this place just twenty years ago, this week. It is true that my communication then was only an oral one and never reduced to writing; for the successful establishment of the Atlantic cable, of which I had received notice that day, called me away suddenly, before the time fixed for the regular presentation; but an elaborate written memoir upon the subject had been presented to the National Academy, ten days previous, at Northampton.

The early history of celestial photography is demonstrably and exclusively American; and its use as a method of delicate quantitative research is very markedly so. Without entering upon the historical data, which are of easy access to every investigator, I may mention that No. 77 of the *Astronomical Journal* contained nineteen photographic impressions of as many different phases of the solar eclipse of 1854, May 26—the moment of each impression being given to the nearest tenth of a second. These were taken at West Point under the direction of Professor Bartlett, of the U. S. Military Academy, and form a part of his memoir, in which he also gives the distances between the cusps, as measured by himself with the micrometer in the telescope. Ten years later, in 1864, Mr. Rutherfurd constructed the $11\frac{1}{4}$ inch photographic object glass which has acquired so conspicuous a place in astronomical history; and with this, in addition to its other achievements, he obtained sharp photographic stellar images with a definition previously unknown, taking for the first time distinct impressions of stars invisible to the naked eye—in fact, to the $8\frac{3}{4}$ magnitude for white stars.

After constructing a micrometer of great delicacy for the measurement of these plates, he measured with this the relative distances and position angles of the stars which they contained. And in the spring of 1866, he kindly placed in my hands the results thus derived from three plates of the Pleiades, each containing two impressions, taken on the evening of March 10.

* Presented at the Buffalo meeting of the American Association for the Advancement of Science, August 20, 1886.

One of these plates contained forty stars. Bessel's memoir upon the Pleiades, published in 1844, gave the relative positions of 54 stars, measured with the Königsberg heliometer, during the years 1829 to 1841. Six of these 54 do not belong within the limits of the plate (which contains about one square degree), and 10 of them are too faint for the photographic record, so that 16 of Bessel's list are wanting; but, on the other hand, there are two additional ones, not observed by him.

From this fact alone it may be perceived that among the great benefits which astronomy may be justified in expecting from celestial photography, the accurate determination of magnitudes does not find place. The chemical action of the stellar light upon the film is so dependent upon the character of that light that, in the absence of a correct knowledge of its composition, we are very easily deceived regarding the amount. Thus one of Bessel's stars which was not recorded upon any of Mr. Rutherfurd's plates is estimated by Argelander as of the magnitude 8·0, and by Wolf as $7\frac{3}{4}$; while five are distinctly recorded which Argelander calls $8\frac{1}{2}$ or less, and eight which Wolf so estimates. The spectroscope would doubtless show a deficiency of the more refrangible rays in the light of the former, and a preponderance of the same in that of the latter.

This series of measurements by Mr. Rutherfurd, together with the computations to which the results were submitted, constitute, if I am not mistaken, the first application of the photographic method to exact astronomical determinations. And the investigation necessarily demanded especial care, both for guarding the numerical results against sources of unsuspected error and for fixing the limits within which known theoretical errors would remain unappreciable.

The importance of the successful application of a method so different from all previous ones, and so full of promise, and also the considerable time which would inevitably elapse before the memoir could be printed, led me at the same time to communicate to the Astronomische Nachrichten, at Altona, some of the resultant values. In a comparatively short note, written about the middle of August, 1866, I gave for the ten most conspicuous stars of the Pleiades, after Alcyone, the corrections derived from one of the photographic plates of March 10, for the values, published by Bessel, of the position-angles and distances from Alcyone in 1840; as likewise the average discordance found for a single measure.

In the next following year the Academy had not the means of printing its memors; and, as, in the mean while, Mr. Rutherfurd had measured five more of the plates of the Pleiades previously taken, as well as six additional ones taken in the months of January and February, 1867, these were also com-

puted, and the results added to those from the first three plates in the memoir already written.

Various circumstances combined to delay the publication, chief among them being what seemed to me a manifest impropriety in printing the results derived from photographs and measurements made by Mr. Rutherfurd, and by his own methods, before some account of these methods should have been published by him. His communication on the subject had been made to the National Academy immediately previous to my own, but was not yet in such form as he desired for publication.

The result showed a very remarkable accordance with Bessel's determination for 1840, although the total amount of relative proper motion during the elapsed 26 years, was comprised in the differences.

This memoir still remains in its original form, but unpublished; the results being deduced from 24 photographic impressions.

In the next year, 1868, I had the gratification of receiving from Mr. Rutherfurd the results of his measurements of 32 stars of the cluster Præsepe, derived from 11 impressions. These were computed in the same way that those of the Pleiades had been, and an analogous memoir upon this cluster was prepared for the National Academy.

Before leaving the country, early in 1870, I gave these two memoirs to Mr. Rutherfurd, with the request that he would send them to the printer at the same time with his own paper, already mentioned; but not before that. The condition of his health prevented him from attending to the matter for some time; and in the interval, he arrived at the unpleasant discovery that the screw of his micrometer had suffered from wear, and to an extent which led him to fear a want of that accuracy of which the method is susceptible, and which he hoped to see demonstrated by its very first applications.

Notwithstanding this possible blemish, it seems to me that the results ought to be now made public in their original form, after due mention of the circumstances; and it is among my hopes to be able soon to publish these two memoirs from the original manuscript of so many years ago.

The method was received with manifest distrust and disregard abroad; and, as was but natural for so essential a deviation from former methods, very many grounds of criticism and objection were brought up. One of the principal of these was the possible distortion of the collodion film, after receiving the impressions and before the measurements; but Mr. Rutherfurd speedily disposed of this point, at least so far as the albuminized plates are concerned; and, moreover, the combination

of measurements of the same stars, derived from various plates, will at once make manifest the degree of confidence to which the several values and their mean are respectively entitled.

A far more serious obstacle to accuracy is presented by the difficulty of obtaining absolutely round images. Irregularity of form in the dots formed by the stellar impressions is almost incompatible with precision of measurement; and, as the time of exposure must often be long, the chief problem was, not so much to obtain the images as to insure uniformity of motion in the telescope during the period of exposure. Not that the photographic processes were not troublesome enough before the introduction of the dry-plate processes, for very great care and numerous precautions were often necessary to prevent the plates from drying too fast; but far the greatest difficulty consisted in obtaining sufficient precision in the clockwork and equatorial motion of the telescope.

It may easily be imagined how great was my desire, when leaving home for South America, to extend this new method of observation to the Southern hemisphere. But the obstacles encountered in the endeavor cannot be easily imagined. Upon these I will not enlarge here further than by saying that, in Cordoba, also, the attainment of circular dots for the star-images offered incomparably the greatest of all the difficulties of a practical character. The time of exposure was limited by the maximum size allowable for the large stars; and, previous to 1878, also by the drying of the plate, although exposures of twenty minutes were not unusual. Nevertheless, by dint of specially constructed governors and regulators, and by ceaseless attention, we did succeed in obtaining impressions which, to the unaided eye, appear absolutely round.

This necessity of long-continued and minute uniformity in the motion of the telescope, is, of course, largely diminished by the employment of instruments of large aperture, inasmuch as the necessary time of exposure is diminished in the same ratio in which the amount of light is increased. It is yet further and most notably diminished by the manifold greater sensitiveness of the dry gelatine plates. But notwithstanding all this, the attainment of round images, while almost indispensable for giving to stellar photography that increased accuracy to which it may lay claim as a means of research in practical astronomy, still demands especial care and precaution.

The Argentine Government cordially afforded every assistance which I deemed it proper to ask for these investigations. And although the chief energies of the Cordoba Observatory were absorbed by those investigations for which the institution was established, I had the satisfaction of obtaining a sufficient number of stellar photographs to occupy not only my own life-

time, but many more, in their measurement and proper computation.

We photographed no northern stars there except the Pleiades and the Præsepe. Of the Pleiades I brought home sixteen plates, with two impressions of the whole group upon each, made in five different years, from 1872 to 1882, inclusive. Although the center of the cluster never attains a greater altitude at Cordoba than 34° 50′, some of the plates contain 70 stars. All but one of Bessel's stars are there, which belong within the limits of the field, the missing one being of the magnitude 9½, and there are yet other stars of the magnitudes 10, 10½, and 11. Of the Præsepe, there are five plates, and with a correspondingly increased number of stars.

About seventy southern clusters have been repeatedly photographed at Cordoba, comprising all those of the southern hemisphere which seemed important; also somewhat more than a hundred double stars, being a sufficient number to serve as a good test of the method. The total number of photographs now on hand is somewhat less than 1,300, only a few having been preserved in which the images were not circular.

Especial attention, however, was given, for many years, to taking frequent impressions, at the proper seasons, of four stars selected, on account of their large proper motion, as likely to manifest appreciable annual parallaxes. The refined and elaborate observations of Drs. Gill and Elkin, at Cape Town, have been made, computed, and published, while the Cordoba photographs have lain untouched in their boxes. There is but one of my four stars, β Hydri, which is not included in their list. Still it will be a matter of much interest to apply the photographic investigation to the same problem, even if for no other purpose than a comparison of the results of the two methods.

I am convinced that the Cordoba plates contain a large number of stars as faint as the eleventh magnitude of Argelander's scale, and believe that these are much the earliest photographs of stars fainter than Mr. Rutherfurd's of 1865 and 1866. There are several plates, covering about a degree square, which cannot contain less than 550 stars, and I believe that some of them contain a greater number. Such are those of the cluster Lac. 4,375 and that near x Carinæ.

The region in the vicinity of η Carinæ, and that magnificent tract in Sagittarius which is too densely sown with stars to be considered merely a portion of the Milky Way, and yet too large and undefined to be regarded simply as a cluster, were both of them taken several times, during the years 1875 to 1882, in series of overlapping photographs, each containing about a square degree, and recorded upon a glass surface of 9

by 12^{cm}. In their present form they are of course of small value for scientific use, inasmuch as the stars are too crowded for their configurations to be easily perceived; and, although these two series form in fact maps of considerable regions in the sky, still the record is of a very perishable nature, and of small avail for use by astronomers until it shall have been translated into an enduring and numerical form by micrometric measurement.

In this connection I may say that one of the greatest of my present anxieties regarding the Cordoba photographs arises from a discovery of the ease with which the collodion or gelatine film may become detached from the glass. The Argentine Government has assigned a moderate sum for the prosecution of the measurements, and with this some progress has already been made. It is but right to add that the full amount was given for which I asked. Still it is now quite inadequate, in consequence of the unfortunate depreciation of the national currency; and, in the present financial crisis there, I cannot reasonably expect more. Yet this matter of prompt measurements appears to me at present much more important than it did while I was unaware of the facility with which the film can blister and peel.

In 1883, after Mr. Common's brilliant success in photographing nebulas with his great three-foot reflector, he proposed to me a joint arrangement for photographing the whole heavens. My work at Cordoba was so near its close that it was out of the question to undertake anything new; but the immense labor requisite for the measurement of the plates would, under any circumstances, have tended to deter me. It is an undertaking demanding the joint energy, application, and material resources of a large number of persons, if the results are to be made available for astronomical use; indeed, I see no other astronomical value in the unmeasured photographs than the possibility of confirming at some future epoch the existence of relative motion previously detected or made probable by some other investigation.

Since then, the process of photographic charting is said to have been systematically undertaken by the Brothers Henry, at Paris. I have seen none of their plates; but their sharpness is highly spoken of, and the work appears to be prosecuted with much skill and very sensitive plates. There can of course be no question as to the value of any permanent record whatsoever, corresponding to a known date; yet I cannot feel that an essential advance is likely to be made in this way until the photographic record shall have been brought within the range of numerical expression.

The measurements of the Cordoba photographs, thus far

completed, are those of the double stars, the four stars with large proper motion, of the Pleiades, of the Præsepe, and of the clusters Lac. 4,375 and Kappa Crucis. The corresponding computations have been made, as yet, only for a portion of the Pleiades impressions, but I am hopeful of completing all these at a comparatively early date. We shall then be able not only to compare the results with Bessel's of 45 years ago, but to test the deduced values of the proper motions by means of the photographic determinations of 1865 and 1866. Meanwhile the valuable memoir of Wolf has been published, giving closely approximate positions for 571 stars of the group, and Dr. Elkin has recently been executing at New Haven a heliometric triangulation of the principal stars. Our photographic results will have to be confronted with his delicate heliometric ones; and should they bear this test with tolerable success, it will be all that can reasonably be desired.

ART. XLIV.—*Lucasite, a new variety of Vermiculite;** by THOMAS M. CHATARD.

IN the course of an examination of the minerals occurring with corundum at Corundum Hill, Macon Co., N. C., a vermiculite has been found which appears to be a distinct variety of this group. The name Lucasite has been given to it in honor of Dr. H. S. Lucas, so well known in connection with the discovery of the Chester, Mass., emery mine and with corundum mining in the South. To him I am indebted for many facilities and much valuable information.

The specimen from which the material examined was taken was found by me *in situ* in a corundum vein, and is a mass of a yellowish brown, small foliated mineral, through the central part of which runs an irregular seam of a fine granular, grass-green mineral, shown by analysis to be actinolite, intermixed with a small quantity of chromite and a large proportion of the vermiculite scales, which are however much smaller than those of the outer portions. The separation was made by means of Thoulet's solution, the specific gravity of the actinolite being 3·062, while that of the Lucasite is 2·613 (in water 25·5°). General characteristics as follows:

Foliated, compact, also disseminated; folia generally small, not over 2^{mm} across. Cleavage basal, eminent. Color, yellowish-brown, the thinnest scales being almost colorless with

* From a paper on "Gneiss-Dunyte Contacts," in the forthcoming Bulletin of the Division of Chemistry, U. S. Geol. Survey, 1885-86. Communicated by permission of the Director.

a greenish tint. Luster submetallic, somewhat greasy, the material having a great resemblance to the so-called "bronze-powder." Easily decomposed by concentrated hydrochloric acid, the silicic acid separating in pearly scales. When heated gives off much water and exfoliates with considerable force, particles being projected several centimeters from the mass, which doubles its bulk and becomes reddish brown with a silvery luster. It was examined microscopically by Mr. J. S. Diller, who reports "that it is biaxial and negative, but the angle between the optic axes, as seen in a cleavage plate split off parallel to the base, is uniformly small. Upon rotating the section, although the cross is plainly distorted into two hyperbolæ, they do not completely separate from each other."

In the following water determination the figures in brackets represent differences, the others being direct determinations:

					Average.
H_2O at 110°	3·87	3·81	3·72	3·72	3·78
H_2O at 130°	0·12	---	---	---	---
H_2O at red heat (blast lamp)	(6·83)	7·04	(6·95)	(7·00)	6·98
	10·82	(10·85)	10·67	10·72	10·76

Of the following analyses Nos. 1 and 2 are the results obtained on the air-dried mineral; No. 3 is the average and No. 4 the average calculated as dried at 110.°

	1.		2.	3.	4.
H_2O at 110°	3·78		(3·78)	3·78	----
H_2O at 130° and r. h.	6·98		(6·98)	6·98	7·22
SiO_2	39·89		39·74	39·81	41·17
Al_2O_3	12·88		----	12·99	13·43
Cr_2O_3	0·54	=18·82	19·04	0·54	0·56
Fe_2O_3	5·29			5·29	5·47
FeO	0·11		----	0·11	0·11
MnO	0·05		0·05	0·05	0·05
CaO	0·14		0·13	0·14	0·14
MgO	24·88		24·78	24·83	25·68
K_2O	5·76		(5·76)	5·76	5·96
Na_2O	0·20		(0·20)	0·20	0·21
	100·50		100·46	100·48	100·00

No. 4 gives the atomic ratio

$\dot{S}i$	:	$\dot{R}$	:	$\dot{R}$	:	$\dot{H}$
2·75	:	1·03	:	1·29	: 0·14	: 0·80
2·75	:		2·46			: 0·80
·7	:		·6			·2

The following table from Professor Cooke's paper* will show the relation of Lucasite to the other members of this group, all being taken as dried at 100°:

	$\dot{S}i$:	$\dot{R}$: $\dot{R}$: $\dot{R}$	: $\dot{H}$
Hallite	2·42 :	2·41	: 1·47 or 8 : 8 : 5 or : 32 : 32 : 20
"Lerni"	2·54 :	2·50	: 1·30 " 2 : 2 : 1 " : 40 : 40 : 20
Pelhamite ..	2·75 :	2·46	: 1·26 " 9 : 8 : 4 " : 45 : 40 : 20
Culsageeite .	2·50 :	2·66	: 1·23 " 2 : 2 : 1 " : 40 : 40 : 20
Jefferisite ...	2·56 :	2·53	: 1·17 " 9 : 9 : 4 " : 45 : 45 : 20
"Milbury" .	2·38 :	2·74	: 1·14 " 8 : 9 : 4 " : 40 : 45 : 20
Lucasite	2·75 :	2·46	: 0·80 " 7 : 6 : 2 " : 70 : 60 : 20

Any attempt to deduce formulas for these minerals gives complicated results, and it is evident that we are dealing with a class of minerals of very similar physical properties but of widely varying compositions. That they are in all cases decomposition products of other minerals is very doubtful, for when the mode of occurrence of these minerals is carefully examined, it is difficult to resist the conclusion that in many cases they are as original in their formation as the chlorites and biotites and are not derived from them. The number of these varieties is already formidable and is constantly being added to, and it is much to be desired that a careful comparison of the whole series shall be made, so as to reduce the number of mineral names and to bring the survivors into a scientific arrangement.

ART. XLV.—*Crystallographic Notes;* by W. G. BROWN.

Artificial Copper Crystals.

ON taking apart to cleanse and put away a cell of a Calland gravity battery, which after using had been put aside and remained undisturbed for two or three months, there was found attached to the insulated wire coming from the copper plate, at the point where it passed the zinc plate, a pendant mass consisting almost entirely of copper crystals. At the place where these crystals were formed, the gutta percha of the insulated wire had been abraded and had come into contact with the zinc plate. The crystals were quite firmly attached to the insulated copper wire, and were also in contact with, but not adherent to, the zinc plate. At the time the crystals were found, the solution in the cell had evaporated to such an extent that about half the deposited crystals were out of the solution, and the zinc plate just touched it.

* Cooke, Proc. Am. Acad., 1875, p. 461.

The crystals were in all probability accidentally formed by electrolysis, a method used by Golding Bird (and before him*), who obtained by means of a modified Daniell cell crystals of copper mixed with crystals of cuprite, a like result to that in the present occurrence, but of the forms present he makes no mention. Kossmann† mentions some electrolytic copper which had the appearance of hexagonal crystals, but were in reality twins, twinning plane the octahedral plane, and cutting each other at angles of 60° and 120°.

The crystals, found under the circumstances described in this note, are in the form of a mass of stout stalactites, with a drusy surface, about 30^{mm} long by 25^{mm} by 15^{mm} and weigh nearly 20 grams.

The upper part of the mass, that first deposited in contact with the zinc, consists of spongy dendritic copper of a loose filamentous texture, of a generally dull chocolate-red color with a metallic sheen. This portion possesses little coherence, readily separates into threads, and is very friable.

Forming a concentric layer round this upper portion are crystals of cuprous oxide (cuprite) mixed with distinctly crystallized compact copper, which forms the outer layer.

The lower and larger portion of the mass consists of definite copper crystals possessing its ordinary color and luster. The forms of the crystal facets, these not exceeding ·2^{mm} at their greatest breadth, cannot be made out without the use of a magnifying glass. When examined in this way the forms present are seen to be those of the octahedron and cube; usually the octahedron alone, but at times with its solid angles replaced by cubic planes. Rarely the cube with the solid angles replaced by the faces of the octahedron elongated into an apparently square prism. Doubtful is the octahedron as a right rhombic prism terminated by a dome, and the rhombic dodecahedron appearing as a hexagonal prism terminated by a rhombohedron. With these are other forms, apparently those of a hexagonal pyramid, due, however, to twinning. By far the greater number of crystals are twins, either according to the spinel law, twinning plane an octahedral face, or the twinning plane that of a trapezohedron forming polysynthetic twins. The most frequent form is one which appears to be a combination of a pentagonal dodecahedron and octahedron, as in pyrite, but this is probably a twin composed of five individuals, the form being that figured by A. von Lasaulx in his paper, "Ueber Zwillingskrystalle von gediegen Kupfer."‡ In many of these crystals four of the components are nearly equally developed,

* See his paper, Phil. Trans., 1837, p. 37.
† Abstract in Chemisches Centralblatt, xvii, 432, 1886.
‡ Zeitschrift für Krystallographie und Mineralogie, viii, 302, fig. 6.

while the fifth is much smaller and its position is such as to show the reëntering angles markedly. Some of the crystals consist of six individuals like fig. 5 in the paper cited, or more like fig. 2 in a paper by Strüver, "Ueber polysynthetische Spinellzwillinge."* Other forms still more complex resemble those of spinel figured in the paper of Strüver referred to.

Artificial crystallized cuprous oxide (Cuprite).

On the outside of the spongy portion of the metallic copper just described there is a more or less continuous purple layer of crystals of cuprous oxide (cuprite). These are individually of a dark ruby-red color, with adamantine to sub-metallic luster.

The forms present are combinations of octahedron and cube in all stages from dominant octahedron to dominant cube. No simple forms were found. These crystals seem to have been formed just when the solution in the cell had evaporated to the level of the top of the deposited copper. They are confined to a very narrow layer near the outside of the dendritic copper, and are not pseudomorphs of the metallic copper, none of the crystals being twinned or in any way resembling in habit those of the metal among which they are found.

Another example of crystallized cuprous oxide (cuprite) was found on a camp spoon picked up near the site of a reserve magazine on Morris Island, South Carolina, constructed by the Federal forces when attacking Battery Wagner, entrance to Charleston harbor, S. C., in 1863. The spoon is of german silver, tarnished dull red and bronze, having spots of a green salt of copper (silicate?) over its surface where small cemented groups and sprinkled grains of sand are firmly attached. Upon the surface of the spoon there are several blisters. Upon opening some of these, that part of the spoon beneath the blister is seen covered with brilliant transparent crystals of cuprous oxide (cuprite), showing only the faces of the octahedron, whose triangular shape can just be distinguished by the unaided eye. With a pocket lens they are distinctly visible.

While there is nothing new in the formation of these crystals on an alloy of copper, the short time in which the crystals have formed seems a noteworthy feature of this occurrence. The spoon could not have been exposed but little more than twenty years, from 1863 to 1886. In all cases mentioned in the books† the articles seem to have been exposed for a very long time to natural agencies, and lead to the conclusion that the formation, for example, on antique bronzes, under special conditions, is a

* Zeitschrift für Krystallographie und Mineralogie, Band ii, p. 480, and Taf. XVII, fig. 2.

† For methods of production and occurrence, see Fouqué et Levy, Synthèse des Minéraux et des Roches, p. 378, and Fuchs Die künstlich dargestellten Mineralien, p. 68.

work of centuries. It may be that the composition of the alloy, german silver, had something to do with the comparative rapidity of production, aided perhaps by the salt water of the sea.

Crystallized Lead carbonate (*Cerussite*).

On the sand near the same locality at which the crystallized cuprous oxide was found, Morris Island, S. C., among the metallic remains of ammunition boxes, wagons, pieces of shell, the *débris* of a battle-field, a dozen Minié balls were picked up.

The greater number of the balls had, upon various parts of the surface patches, of lead carbonate, sometimes amorphous, sometimes acicular, crystalline and transparent, in some places 1^{mm} thick. It was seen, however, that more was present in the cup and about the base of the ball than elsewhere. The balls had not been fired, and were probably dropped attached to the cartridge, and it is possible that the formation of the carbonate was hastened by the rapid oxidation of the lead by the nitre of the gunpowder of the cartridge in the presence of moisture.

The Citadel, S. C. M. A., Charleston, S. C., Aug. 17, 1886.

ART. XLVI.—*On the chemical composition of Ralstonite;* by S. L. PENFIELD and D. N. HARPER.

A NUMBER of years ago Professor Geo. J. Brush generously gave one of us a specimen containing a large amount of the rare mineral ralstonite associated with thomsenolite from Arksut Fiord, Greenland. Considerable time was spent in picking out the octahedral crystals free from thomsenolite and only a partial analysis was made which was never published; difficulty was found in determining the fluorine, and the material was exhausted before a complete analysis was made. The results were essentially the same as those of Nordenskiöld.* Since then J. Brandl† has analyzed the mineral, using material which was selected by Professor P. Groth. The results of the three analyses are as follows:

	Nordenskiöld.	Penfield.	Brandl.
Mg	5·52	4·29	3·56
Na	5 66	4·12	5·50
K	*tr.*	·11	----
Ca	1·99	1·67	1·53
Al	22·94	22·33	22·14
H_2O	14·84	18·41	10·00
F	----	----	57·12
	49·95		99·85

* Geol. Fören. i Stockholm. Förhandl., 1874, ii, 81.

† Annalen der Chemie, ccxiii, 7.

Only small quantities were used in making the above analyses owing to the scarcity of material and the difficulty of obtaining it free from thomsenolite. As far as the metals are concerned the three analyses agree remarkably well with one another. Nordenskiöld calculated that the 50·05 per cent of material which was not determined in his analysis, if assumed to be fluorine would not be sufficient to satisfy all the metals, and concludes that the mineral must contain some oxygen. The same would be true in Penfield's analysis. Brandl determined the fluorine directly and found it just sufficient to satisfy the metals; he proposed the formula $3(Na_2MgCa)F_2$, $8AlF_3$, $6H_2O$.

The fragments which were left after selecting material for the above analysis by Penfield were carefully saved, and although we could see that they contained large quantities of ralstonite, still it was found impossible to separate the pure mineral from thomsenolite by hand picking. After selecting the purest crystal of ralstonite we could find, we took its specific gravity by just floating it in the Thoulet solution and found it to be 2·560; by the same method we found the specific gravity of cryolite to be 2·974 and thomsenolite 2·979. The great difference in specific gravity between ralstonite and the other two minerals admits of a very perfect separation by means of the Thoulet solution. All the material which we had was crushed and made to pass an eighty-mesh sieve, the finest dust was washed away by means of water and the separation carried on as usual, repeating it several times so as to remove the last traces of thomsenolite. Finally, the purest powder was floated on a solution whose specific gravity was 2·611, a few heavier particles were removed and the solution diluted to the specific gravity 2·551, when all but a trace of the powder sank to the bottom. This material, amounting to over twelve grams, showed no impurity of double refracting thomsenolite when examined under the polarizing microscope, and was used in making the following analysis.

A few remarks are necessary regarding the method of analysis. It was found to be practically impossible to decompose the mineral completely with sulphuric acid; a residue was always left which could not be dissolved by further treatment with sulphuric acid, nor with hydrochloric or nitric acids. For the determination of the metals the mineral was fused with sodium carbonate, the fusion soaked out with water, sulphuric acid was added, and the solution evaporated and ignited till the hydrofluoric and excess of sulphuric acids were expelled. The mass was then dissolved in water and the metals determined according to the usual methods. Sodium was determined once by Smith's fusion method, and again by decomposing the min-

eral as far as possible with sulphuric acid, assuming that the sodium was all in solution and the insoluble portion some compound of aluminum. Water was determined by igniting the mineral in a combustion tube, passing the vapor over dry sodium carbonate and collecting it in a weighed chloride of calcium tube. The fluorine could not be determined by driving off the silicon fluoride. About twenty-five per cent of fluorine was readily driven off; by continuing the decomposition with the temperature of the sulphuric acid about 160°, silicon fluoride was slowly, but, in the course of six or eight hours never completely given off. The greatest amount of fluorine which we were able to drive over by this method was about thirty-one per cent. This is not at all in accordance with the statement of Brandl,* who states that the decomposition commences at 145° and is completed at 160°. We found it impossible to make a determination according to the method described by him. Our determinations are made according to the Berzelius method by fusing with mixed potassium and sodium carbonates and silica. They are probably too low by about one-half to one per cent, judging from test experiments which we made on cryolite. Our determinations are as follows:

			Mean.	Ratio.		
Mg	4·46	4·31	4·39	·183		
Na	4·25	4·27	4·27÷46	·093	·278	1·00
K	·12	·12	·12÷78	·002		
Ca	·03	---	·03			
Al	24·23	24·27	24·25		·882	3·17
F	39·76	40·05	39·91		2·101	7·56
H_2O	18·72	18·74	18·73			
			91·70			

The ratio of $(MgNa_2K_2) : Al = 1 : 3$ nearly. This ratio being assumed as correct, the ratio of the fluorine necessary to unite with the metals should be 11, whereas we only find 7·56. The fluorine is therefore not sufficient to unite with the metals and this is fully in accordance with the suggestion of Nordenskiöld. If the metals in our analysis, which are in excess of the fluorine, are united to hydroxyl, which, as has been shown to be the case in several instances, is capable of replacing fluorine, it would be necessary, in order to make the ratio $(Mg, Na_2, K_2) : Al : (F+OH) = 1 : 3 : 11$, to have 16·27 per cent of hydroxyl corresponding to 8·61 per cent of water; the remaining 10·12 per cent of water would then be regarded as water of crystallization, and would correspond to two molecules, making the formula of the mineral $(Mg, Na_2)Al_3(F+OH)_{11} . 2H_2O$. Making this disposition of the water, our analysis would be:

* Loc. cit.

		Ratio.		
Mg	4·39	·183		
Na	4·27	·093	·278	1·00
K	·12	·002		
Ca	·03			
Al	24·25		·882	3·17
F	39·91	2·101		
OH	16·27	·957	3·058	11·00
H_2O	10·12		·562	2·02
	99·36			

It will be seen that the assumption that hydroxyl replaces fluorine not only makes up for the deficiency in the analysis but also leads to a very satisfactory ratio. This assumption is also well supported by actual experiment. When the mineral is cautiously heated in a closed glass tube at first neutral water, by stronger ignition acid water, is driven off. The first that comes off is undoubtedly water of crystallization, afterwards the hydroxyl is decomposed and fluorine comes off in combination with the hydrogen. By drying the air-dry powder at 100° C. there is a loss of only 0·10 per cent; by heating in an air bath to a temperature never exceeding 250° C. the mineral lost 10·37 per cent; the water goes out very slowly; the experiment was carried on for over a week, during the last three days of which the weight remained very constant. If this 10·37 per cent is regarded as water of crystallization, the remaining 8·36 per cent would correspond to 15·78 per cent of hydroxyl, which agree closely with the figures in our last analysis. The ratio of Mg : Na is almost exactly 1 : 1, there seems to be no simple ratio between F and OH. The excess of the aluminum in the analysis may be owing to some slight impurity. We have never seen perfectly transparent, glassy crystals of ralstonite, and their turbidity may be owing to some slight decomposition; if this is the case the alkalies would naturally be most readily removed, causing the aluminum to be too high. If our fluorine determination should be as much as one per cent too low, which is probably not the case, our results would not be materially changed. Using the actual water of crystallization and hydroxyl determinations and determining the fluorine by difference, we would have for the latter part of our analysis:

		Ratio.		
F	40·79	2·147		
OH	15·78	·928	3·075÷278	11·06
H_2O	10·37		·576÷278	2·07

Probably this determination of fluorine by difference 40·79 per cent represents the true amount of that element more closely than the results of our actual determinations.

Assuming, as it seems fair to do, that our results and conclusions are correct and that the formula which we have proposed is the true one, namely, that the mineral is an isomorphous mixture of $(MgNa_2)Al_3F_{11}, 2H_2O$ and $(MgNa_2)Al_3(OH)_{11}, 2H_2O$, in which formulæ fluorine and hydroxyl play the same part or are isomorphous, let us see if we can in any way account for the variations in the previously published analyses, especially between Brandl's and our own, the only two complete analyses. First, we would emphasize that the greatest care was used in preparing the material for our analysis; the extremes in the specific gravity of the powder which we separated were 2·611 and 2·551, or a variation between the lightest and heaviest of only 0·060. Second, our analysis shows that our material is practically free from calcium, indicating a very complete separation from thomsenolite with which the ralstonite is so intimately associated, and showing that calcium is not an essential constituent of the mineral. Third, we were not limited regarding the amount of material which we could use, as we had an abundant supply of the pure mineral. From the same specimen from which our material was derived, one of us by very careful picking was able to obtain nearly one gram of octahedral crystals, which he supposed were pure but which, as is shown in the analysis near the beginning of this article, contained 1·67 per cent of calcium, showing that a most careful and laborious hand-picking had not been sufficient to free the small crystals wholly from thomsenolite from which the calcium was unquestionably derived. It seems highly probable that other investigators have worked with material containing slight quantities of thomsenolite. Groth,* for instance, states that the material which he furnished to Brandl for analysis, showed under the polarizing microscope particles of a strongly double refracting mineral with quadratic habit which was unquestionably thomsenolite. If we assume that the mineral is free from calcium as our analysis indicates and that the calcium in the other analyses is all derived from thomsenolite, we should find by calculation the following figures giving the per cent of thomsenolite in the analyzed material and of the mineral free from thomsenolite.

	Nordenskiöld.	Penfield.	Brandl.	Calculated from $(MgNa_2)Al_3F_{11}.2H_2O$
Thomsenolite	11·07	9·28	8·51	----
Mg	6·20	4·29	3·90	4·46
Na	3·95	4·27	5·05	4·27
Al	24·27	21·19	23·06	22·99
F	----		57·68	58·25
H_2O	15·68	19·46	10·17	10·03
			99·85	100·00
Ratio $(MgNa_2) : Al =$	1·17 : 3	1·06 : 3	0·97 : 3	

* Zeitschr. Krvst., vii, 474.

In these analyses the ratio $(MgNa_2)$: Al = 1 : 3 very nearly, especially considering the small quantity of mineral which was used in making the analyses and in Brandl's analysis, the only complete one, the fluorine is just sufficient to satisfy the metals, while the ratio $(MgNa_2)$: Al : F : H_2O = 0·97 : 3 : 10·88 : 2·02, or nearly 1 : 3 : 11 : 2, the same as required by the formula proposed by us, only in this case the compound is a pure fluorine compound, containing no hydroxyl. Following Brandl's analysis we have given the percentage composition calculated from the above ratio in which the Mg : Na = 1 : 1. It will be noticed how closely the figures agree with the analysis of Brandl.

The great difference in the proportion in which the metals are united, $(MgNa_2)$: Al = 1 : 3 in ralstonite and Ca : Na : Al = 1 : 1 : 1 in thomsenolite, would account for the decided change in the formula derived from an analysis of a mixture of ralstonite with a little thomsenolite. Brandl's formula, $3(Na_2MgCa)F_2$, $8AlF_3$, $6H_2O$, is therefore a little too low in AlF_3. The difference in the deportment of the mineral when treated with strong sulphuric acid may be owing to the fact that although the fluorine compound is readily decomposed by that acid so that Brandl was able to determine the fluorine by driving over SiF_4, the hydroxyl compound in our mineral in some way hinders the decomposition of the fluorine compound, perhaps by being in itself with difficulty decomposed and inclosing and thus protecting some of the molecules of the fluorine compound from decomposition.

In thin sections under the microscope all of the ralstonite appears very transparent and free from visible inclusions and decomposition products. Some of the crystals on the original specimen were colored yellow and where one of these had been cut through, the yellow substance was seen to consist of a very thin film, probably of iron oxide, coating the crystal. The larger crystals were zonal in structure, the zones lying parallel to the faces of the octahedron. This zonal structure is scarcely perceptible in ordinary light, being indicated by faint greyish streaks running parallel to the contours of the cross section, which could not be resolved by the use of high powers into visible inclusions. In polarized light the zonal structure was more perceptible; all of the crystals show slight double refraction and a division of the cross sections into fields reminding one of the double refraction of analcite. The slightly double refracting ralstonite with its absence of cleavage is in marked contrast to the strongly double refracting thomsenolite, showing brilliant polarization colors, blue of the second order, distinct cleavage and inclined extinction.

In closing we wish to express our thanks to Professor Geo. J. Brush for his liberality in providing us with the rare material for carrying on this investigation.

Mineralogical Laboratory, Sheffield Scientific School, Oct. 12th, 1886.

Art. XLVII.—*Mineralogical Notes;* by Edward S. Dana.

1. *Columbite from Standish, Maine.**

The crystals of columbite obtained a few years since from Standish, Maine, largely through the active efforts of Professor O. D. Allen, are well known for their brilliant luster and perfection of form. In these respects they are far superior to those from any other known locality. This fact has made it seem worth while to attempt a more exact determination of the crystalline elements than has been hitherto possible. The material in hand consisted of a large suite of crystals belonging to the Yale College cabinet and that of Professor Brush, and also a number of fine specimens kindly loaned for the purpose by Dr. George H. Williams of the Johns Hopkins University in Baltimore.

The true form of the species was first established by J. D. Dana, fifty years since, and the axial ratio was determined by him on a crystal from Middletown with considerable exactness. In 1861 Schrauf published his monograph of the species and in it gave a new value of the axes upon the basis of measurements of crystals from Greenland and Bodenmais. He also adopted another fundamental form, making u(133 Dana) the unit pyramid. Schrauf's axes have been accepted since that time, although a comparison of his measured and calculated angles shows that the material he worked upon was not suited to give very accurate results.

Among the crystals from Standish a few were found allowing of exact measurement and the angles obtained from the best one of these are given in the following table:

	Calculated.		Measured.	
$530 : 5\bar{3}0$	52°	52′	52°	42′
$\bar{5}30 : \bar{5}\bar{3}0$			52	53
$530 : \bar{5}30$	127	8	127	9
$5\bar{3}0 : \bar{5}30$			127	14
$110 : 1\bar{1}0$	79	17	79	14
$\bar{1}10 : \bar{1}\bar{1}0$			79	18
$110 : \bar{1}10$	100	43	100	45
$1\bar{1}0 : \bar{1}10$			100	43
$130 : 1\bar{3}0$	136	10	*136	10
$\bar{1}30 : \bar{1}\bar{3}0$			130	10
$130 : \bar{1}30$	43	50	43	51
$1\bar{3}0 : \bar{1}30$			43	49
$001 : 021$	60	40	60	41
$021 : 0\bar{2}1$	121	20	——	
$001 : 103$	19	42	——	
$111 : \bar{1}11$	108	43	*108	43
$1\bar{1}1 : \bar{1}11$			108	42
$111 : 1\bar{1}1$	62°	28′	62°	20′
$\bar{1}11 : \bar{1}\bar{1}1$			62	29
$111 : \bar{1}11$	77	29	77	23
$1\bar{1}1 : \bar{1}\bar{1}1$			77	36
$233 : \bar{2}33$	56	17	——	
$133 : \bar{1}\bar{3}3$	87	36	——	
$\bar{1}33 : \bar{1}33$				
$133 : \bar{1}33$	79	54	——	
$\bar{1}33 : \bar{1}\bar{3}3$				
$133 : \bar{1}33$	29	57	29	59
$\bar{1}33 : \bar{1}\bar{3}3$				
$163 : \bar{1}63$	118	20	118	19
$\bar{1}63 : \bar{1}\bar{6}3$			——	
$163 : \bar{1}63$	19	54	19	59
$\bar{1}63 : \bar{1}\bar{6}3$			20	0

* Abstract of an article published in vol. xii of Groth's Zeitschrift für Krystallographie und Mineralogie.

As fundamental angles the following (indicated by an asterisk in the table) were accepted:

$$130 \wedge 1\bar{3}0 = 136° 10' \quad \text{and} \quad 111 \wedge \bar{1}\bar{1}1 = 108° 43'$$

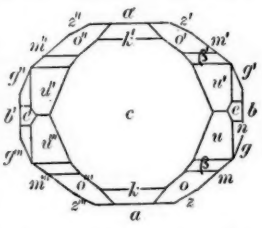
The axial ratio deduced from these is given below, and the above table contains the calculated angles:

$\breve{a} : \bar{b} : c = 0{\cdot}82850 : 1 : 0{\cdot}88976$ (Dana's position).
$a : b : c = 0{\cdot}40234 : 1 : 0{\cdot}35798$ (Schrauf's position).

A comparison of the measured and calculated angles shows a very satisfactory agreement considering the character of the species and the results previously obtained. Measurements on a number of other less perfect crystals gave confirmatory results.

No reason was found for believing that there is any deviation from the orthorhombic form such as has been often suggested, and in fact it was proved that no deviation greater than two or three minutes could exist. Arzruni and Groth have shown on optical grounds that columbite is really orthorhombic. The axial ratio here deduced agrees much better with that of J. D. Dana (in fact the angles of the prismatic zone are nearly identical) than with that of Schrauf. The similarity in angles between the Middletown and Standish crystals goes to prove that the variation of form connected with the change of composition must be small. The Middletown columbite contains a large amount of iron tantalate (nearly 30 per cent), while the Standish mineral is nearly pure iron columbate (niobate) with the specific gravity $=5{\cdot}65$. As bearing in the same direction the writer found that the Northfield columbite, which is almost a tantalite (57 per cent Ta_2O_5, sp. grav. $=6{\cdot}8$), had also the habit and angles of ordinary columbite.

The crystals of columbite from Standish resemble those of Haddam and Middletown in habit, though occasionally they are short and stout, somewhat elongated in the direction of the $\breve{a}$ axis and hence approximate to the Greenland form. The accompanying figure is a basal projection of a common type. Twins are common and all after the usual law, the twinning plane the brachydome *e*. They are generally the familiar heart-shaped contact twins, but much variation in habit is observed. The observed planes on the Standish crystals are:



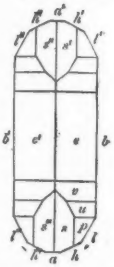
a (100, $i\text{-}\bar{i}$), *b* (010, $i\text{-}\bar{i}$), *c* (001, *O*), *z* (530, $i\text{-}\frac{5}{3}$), *m* (110, *I*), *g* (130, $i\text{-}3$), *l* (106, $\frac{1}{6}\text{-}\bar{i}$), *k* (103, $\frac{1}{3}\text{-}\bar{i}$), *e* (021, $2\text{-}\bar{i}$), *o* (111, 1), β (233, $1\text{-}\frac{3}{2}$), *u* (133, $1\text{-}3$), *n* (163, $2\text{-}6$).

2. *Diaspore.*

Diaspore from Newlin, Penn.—Through the kindness of Professor F. W. Clarke and Mr. Joseph Willcox I have had the opportunity of examining a crystal of diaspore from Newlin, near Unionville, Penn., belonging to the latter. The crystal is the finest ever found at the locality; it is prismatic in habit, about $\frac{3}{8}$ inch in length and doubly terminated. It showed the following planes, the vertical zone being strongly striated: b (010, i-$\bar{1}$), h (210, i-$\bar{2}$), l (120, i-$\frac{1}{2}$), n (150, i-$\frac{1}{5}$), q (232, $\frac{3}{2}$-$\frac{3}{2}$). Of these the pyramid q is new to the species; such measurements as were possible agreed with the calculated angles (accepting the axes of Kokscharow). Better measurements were obtained on a similar but very small crystal from the same locality in the cabinet of Professor Brush. These angles are:

	Calculated.	Measured.
$qq' = 232 \wedge \bar{2}32 =$	51° 2′	51° 20′
$qq'' = 232 \wedge 2\bar{3}2 =$	96° 3′	96° 14′
$qq''' = 232 \wedge 23\bar{2} =$	74° 34′	74° 30′

Diaspore from Chester, Mass.—The Chester diaspore commonly occurs in thin cleavage plates implanted upon the magnetite gangue. Occasionally it is found in very delicate acicular crystals, and again in groups of crystals tabular parallel to the brachypinacoid. A basal projection of the last named form is given in the accompanying figure. The crystals are in fact composite, consisting of parts in nearly parallel position. They are deeply striated in the vertical zone and also in the zone *pe*. The observed planes are:



a (100, i-$\bar{1}$), b (010, i-$\bar{1}$), h (210, i-$\bar{2}$), l (120, i-$\frac{1}{2}$), e (011, 1-$\bar{1}$), s (212, 1-$\bar{2}$), p (111, 1), u (344, 1-$\frac{4}{3}$), v (122, 1-$\frac{1}{2}$). Of these u and v are new, they are determined by the following measurements:

	Calculated.	Measured.
$au =$	67° 30′	67° 29′
$av =$	74° 35′	75° 30′ approx.

3. *Zincite.*

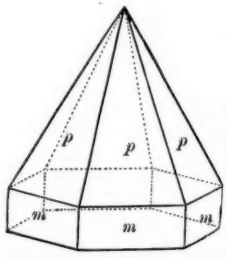
Our knowledge of the crystals of zinc protoxide has rested thus far upon observations of artificial crystals, the native mineral showing usually only massive forms with distinct basal and prismatic cleavage. Early measurements of the artificial crystals were given by Rose (in 1852), and since then contributions to the subject have been made by vom Rath, DesCloizeaux

and others. The recent observations of Rinne (Jahrb. Min., 1884, ii, 164) appear to be highly accurate. He obtained as the pyramidal angle 52° 21′ and deduced as the vertical axis:

$$c = 1{\cdot}6219$$

His observations upon etching-figures and other related points are also of much interest.

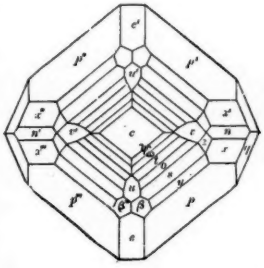
The cabinet of Mr. Clarence S. Bement contains several specimens of the zincite from Stirling Hill, N. J., which show distinct crystals. In two of these the acute quartzoid characteristic of the species is seen in hemimorphic development with the prism and basal plane at one extremity. Other specimens show the same form larger (up to two inches in length) in outline imbedded in the gangue. The accompanying figure gives the habit of the crystals, and the following angles, though making no claim to accuracy, show a fair agreement with the angles of the artificial forms.



	Measured.		Calculated.	
pp'	= 53°	43′	52°	21′ Rinne.
$p'p''$	= 54°	2′		
cp	= 65°	20′ approx.	61°	54′

4. *Sulphur.*

A specimen of native sulphur from Rabbit Hollow, Nevada, in the cabinet of Professor Brush, is interesting because of the complexity of the form. The mass is crystalline throughout, the parts mostly in parallel position, though distinct isolated crystals are rare. One of these was detached and measured; it is shown in the figure in basal projection. The planes are:



c (001, o), u (103, $\frac{1}{3}$-$\bar{1}$), e (101, 1-$\bar{1}$), v (013, $\frac{1}{3}$-$\bar{1}$), n (011, 1-$\bar{1}$), ψ (119, $\frac{1}{9}$), ω (118, $\frac{1}{8}$), t(115, $\frac{1}{5}$), o(114. $\frac{1}{4}$), s(113, $\frac{1}{3}$), y (112, $\frac{1}{2}$), p (111, 1), β (315, $\frac{3}{5}$-$\bar{3}$), z (135, $\frac{3}{5}$-$\bar{3}$), x (133, 1-$\bar{3}$). Of these β and o are new; β was determined by the zones p, s''' (111, 1$\bar{1}$3) and y, x (112, 133); also o lies in the zone c, p (001, 111).

The calculated angles of these forms (accepting the axes of Kokscharow) are:

	Calculated.	Measured.
$oo' = 315 \wedge 3\bar{1}5 =$	24° 54′	
$co = 001 \wedge 114 =$	37° 2′	37° 1′

Other measured angles are:

	Calculated.	Measured.
$ct = 001 \wedge 115 = 31°\ 6'$		$31°\ 9'$
$cs = 001 \wedge 113 = 45°\ 10'$		$45°\ 9'$
$cy = 001 \wedge 112 = 56°\ 28'$		$56°\ 26'$
$cp = 001 \wedge 171 = 71°\ 40'$		$71°\ 40'$

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *Geological Time. Address of Professor* G. H. Darwin *before the Mathematical and Physical Section of the British Association at Birmingham.*— . . . It will, I think, be useful to avail myself of the present occasion to direct your attention to a certain group of theories which are still in an undeveloped and somewhat discordant condition, but which must form the nucleus round which many observations have yet to be collected before these theories and their descendants can make a definitely accepted body of truth. If I am disposed to criticise some of them in their actual form, I shall not be understood as denying the great service which has been rendered to science by their formulation.

Great as have been the advances of geology during the present century, we have no precise knowledge of one of its fundamental units. The scale of time on which we must suppose geological history to be drawn is important not only for geology itself, but it has an intimate relation with some of the profoundest questions of biology, physics, and cosmogony.

We can hardly hope to obtain an accurate measure of time from pure geology, for the extent to which the events chronicled in strata were contemporaneous is not written in the strata themselves, and there are long intervals of time of which no record has been preserved.

An important step has been taken by Alfred Tylor, Croll, and others, towards the determination of the rate of action of geological agents. From estimates of the amount of sediment carried down by rivers, it appears that it takes from 1000 to 6000 years to remove one foot of rock from the general surface of a river basin.

From a consideration of the denuding power of rivers and a measurement of the thickness of stratified rock, Phillips has made an estimate of the period of time comprised in geological history, and finds that, from stratigraphical evidence alone, we may regard the antiquity of life on the earth as being possibly between 38 and 96 millions of years ("Life on the Earth," Rede Lecture, 1860).

Now while we should perhaps be wrong to pay much attention to these figures, yet at least we gain some insight into the order of magnitude of the periods with which we have to deal, and we may feel confident that a million years is not an infinitesimal fraction of the whole of geological time.

It is hardly to be hoped, however, that we shall ever attain to any very accurate knowledge of the geological time scale from this kind of argument.

But there is another theory which is precise in its estimate, and which, if acceptable from other points of view, will furnish exactly what is requisite. Mr. Croll claims to prove that great changes of climate must be brought about by astronomical events of which the dates are known or ascertainable ("Climate and Time"). The perturbation of the planets causes a secular variability in the eccentricity of the earth's orbit, and we are able confidently to compute the eccentricity of many thousands of years forward and backward from to-day, although it appears that, in the opinion of Newcomb and Adams, no great reliance can be placed on the values deduced from the formulæ at dates so remote as those of which Mr. Croll speaks. According to Mr. Croll, when the eccentricity of the earth's orbit is at its maximum, that hemisphere which has its winter in aphelion would undergo a glacial period. Now, as the date of great eccentricity is ascertainable, this would explain the great Ice Age and give us its date.

The theory has met with a cordial acceptance on many sides, probably to a great extent from the charm of the complete answer it affords to one of the great riddles of geology.

Adequate criticism of Mr. Croll's views is a matter of great difficulty on account of the diversity of causes which are said to coöperate in the glaciation. In the case of an effect arising from a number of causes, each of which contributes its share, it is obvious that if the amount of each cause and of each effect is largely conjectural, the uncertainty of the total result is by no means to be measured by the uncertainty of each item, but is enormously augmented. Without going far into details it may be said that these various concurrent causes result in one fundamental proposition with regard to climate, which must be regarded as the keystone of the whole argument. That proposition amounts to this—that climate is unstable.

Mr. Croll holds that the various causes of change of climate operate *inter se* in such a way as to augment their several efficiencies. Thus the trade-winds are driven by the difference of temperature between the frigid and torrid zones, and if from the astronomical cause the northern hemisphere becomes cooler the trade-winds on that hemisphere encroach on those of the other, and the part of the warm oceanic current which formerly flowed into the cold north zone, will be diverted into the southern hemisphere. Thus the cold of the northern hemisphere is augmented, and this in its turn displaces the trade-winds further, and this again acts on the ocean currents, and so on; and this is neither more nor less than instability.

But if climate be unstable, and if, from some of those temporary causes for which no reasons can as yet be assigned, there occurs a short period of cold, then surely some even infinitesimal

portion of the second link in the chain of causation must exist; and this should proceed in the first case to augment the departure from the original condition, and the climate must change.

In a matter so complex as the weather, it is at least possible that there should be instability when the cause of disturbance is astronomical, whilst there is stability in an ordinary sense. If this is so, it might be explained by the necessity for a prolonged alteration in the direction of prevailing winds in order to affect oceanic currents (Zöppritz, *Phil. Mag.*, 1878).

However this may be, so remarkable a doctrine as the instability of climate must certainly be regarded with great suspicion, and we should require abundant proof before accepting it. Now there is one result of Mr. Croll's theory which should afford almost a crucial test of its acceptability. In consequence of the precession of the equinoxes the conditions producing glaciation in one hemisphere must be transferred to the other every 10,000 years. If there is good geological evidence that this has actually been the case, we should allow very great weight to the astronomical theory, notwithstanding the difficulties in its way. Mr. Croll has urged that there is such evidence, and this view has been recently strongly supported by M. Blytt (*Nature*, July 8 and 15, 1886). Other geologists do not, however, seem convinced of the conclusiveness of the evidence.

Thus Mr. Wallace ("Island Life"), whilst admitting that there was some amelioration of climate from time to time during the last glacial period, cannot agree in the regular alternations of cold and warm demanded by Mr. Croll's theory. To meet this difficulty he proposes a modification. According to his view, large eccentricity in the earth's orbit will only produce glaciation when accompanied by favorable geographical conditions. And when extreme glaciation has once been established in the hemisphere which has its winter in aphelion, the glaciation will persist, with some diminution of intensity, when precession has brought round the perihelion to the winter. In this case, according to Wallace, glaciation will be simultaneous on both hemispheres.

Again, he contends that, if the geographical conditions are not favorable, astronomical causes alone are not competent to produce glaciation.

There is agreement between the two theories in admitting instability of climate at first, when glaciation is about to begin under the influence of great eccentricity of the orbit, but afterwards Wallace demands great stability of climate. Thus he maintains that there is great stability in extreme climates, either warm or cold, while there is instability in moderate climates. I cannot perceive that we have much reason from physical considerations for accepting these remarkable propositions, and the acceptance or rejection of them demands an accurate knowledge of the most nicely balanced actions, of which we have as yet barely an outline.

Ocean currents play a most prominent part in these theories,

but at this moment our knowledge of the principal oceanic circulation, and of its annual variability, is very meagre. In the course of a few years we may expect a considerable accession to our knowledge, when the Meteorological Office shall have completed a work but just begun—viz., the analysis of ships' logs for some sixty years, for the purpose of laying down in charts the oceanic currents.

With regard to the great atmospheric currents even the general scheme is not yet known. Nearly thirty years ago Professor James Thomson gave before this Association at Dublin an important suggestion on this point. As it has been passed over in complete silence ever since, the present seems to be a good opportunity of redirecting attention to it.

According to Halley's theory of atmospheric circulation, the hot air rises at the equator and floats north and south in two grand upper currents, and it then acquires a westward motion relatively to the earth's surface, in consequence of the earth's rotation. Also the cold air at the pole sinks and spreads out over the earth's surface in a southerly current, at first with a westerly tendency, because the air comes from the higher regions of the atmosphere, and afterwards due south, and then easterly, when it is left behind by the earth in its rotation.

Now Professor Thomson remarks that this theory disagrees with fact in as far as that in our latitudes, the winds, though westerly, have a poleward tendency, instead of the reverse.

In the face of this discrepancy he maintains that "the great circulation already described does actually occur, but occurs subject to this modification, that a thin stratum of air on the surface of the earth in the latitudes higher than 30°—a stratum in which the inhabitants of those latitudes have their existence, and of which the movements constitute the observed winds of those latitudes—being by friction and impulses on the surface of the earth, retarded with reference to the rapid whirl or vortex-motion from west to east of the great mass of air above it, tends to flow toward the pole, and actually does so flow to supply the partial void in the central parts of that vortex due to the centrifugal force of its revolution. Thus it appears that in the temperate latitudes there are three currents at different heights—that the uppermost moves toward the pole, and is part of a grand primary circulation between equatorial and polar regions; that the lowermost moves also toward the pole, but is only a thin stratum forming part of a secondary circulation; that the middle current moves from the pole, and constitutes the return current for both the preceding; and that all these three currents have a prevailing motion from west to east (Brit. Assoc. *Report*, Dublin, 1857, pp. 38, 39).

Such, then, appears to be our present state of ignorance of these great terrestrial actions, and any speculation as to the precise effect of changes in the annual distribution of the sun's heat must

be very hazardous until we know more precisely the nature of the thing changed.

When looking at the astronomical theory of geological climate as a whole, one cannot but admire the symmetry and beauty of the scheme, and nourish a hope that it may be true; but the mental satisfaction derived from our survey must not blind us to the doubts and difficulties with which it is surrounded.

And now let us turn to some other theories bearing on this important point of geological time.

Amongst the many transcendent services rendered to science by Sir William Thomson, it is not the least that he has turned the searching light of the theory of energy on to the science of geology. Geologists have thus been taught that the truth must lie between the cataclysms of the old geologists and the uniformitarianism of forty years ago. It is now generally believed that we must look for a greater intensity of geologic action in the remote past, and that the duration of the geologic ages, however little we may be able mentally to grasp their greatness, must bear about the same relation to the numbers which were written down in the older treatises on theology, as the life of an ordinary man does to the age of Methuselah.

The arguments which Sir William Thomson has adduced in limitation of geological time are of three kinds. I shall refer first to that which has been called the argument from tidal friction; but before stating the argument itself it will be convenient to speak of the data on which the numerical results are based.

Since water is not frictionless, tidal oscillations must be subject to friction, and this is evidenced by the delay of twenty-four to thirty-six hours which is found to occur between full and change of moon and spring-tide. An inevitable result of this friction is that the diurnal rotation of the earth must be slowly retarded, and that we who accept the earth as our timekeeper must accuse the moon of a secular acceleration of her motion round the earth, which cannot be otherwise explained. It is generally admitted by astronomers that there actually is such an unexplained secular acceleration of the moon's mean motion.

No passage in Thomson and Tait's "Natural Philosophy" has excited more general interest than that in which Adams is quoted as showing that, *with a certain value for the secular acceleration*, the earth must in a century fall behind a perfect chronometer, set and rated at the beginning of the century, by twenty-two seconds. Unfortunately this passage in the first edition gave an erroneous complexion to Adams's opinion, and being quoted without a statement of the premises, has been used in popular astronomy as an authority for establishing the statement that the earth is actually a false timekeeper to the precise amount specified.

In the second edition (in the editing of which I took part) this passage has been rewritten, and it is shown that Newcomb's estimate of the secular acceleration gives only about one-third of

the retardation of the earth's rotation, which resulted from Adams's value. The last sentence of the paragraph here runs as follows: "It is proper to add that Adams lays but little stress on the actual numerical values which have been used in this computation, and is of opinion that the amount of tidal retardation of the earth's rotation is quite uncertain." Thus, in the opinion of our great physical astronomer, a datum is still wanting for the determination of a limit to geological time, according to Thomson's argument.

However, subject to this uncertainty, with the values used by Adams in his computation, and with the assumption that the rate of tidal friction has remained constant, then a thousand million years ago the earth was rotating twice as fast as at present. In the last edition of "Natural Philosophy" the argument from these data runs thus:

"If the consolidation of the earth took place then or earlier, the ellipticity of the upper layers (of the earth's mass) must have been $\frac{1}{230}$ instead of about $\frac{1}{300}$, as it is at present. It must necessarily remain uncertain whether the earth would from time to time adjust itself completely to a figure of equilibrium adapted to the rotation. But it is clear that a want of complete adjustment would leave traces in preponderance of land in equatorial regions. The existence of large continents and the great effective rigidity of the earth's mass render it improbable that the adjustment, if any, to the appropriate figure of equilibrium would be complete. The fact, then, that the continents are arranged along meridians, rather than in an equatorial belt, affords some degree of proof that the consolidation of the earth took place at a time when the diurnal rotation differed but little from its present value. It is probable, therefore, that the date of consolidation is considerably more recent than a thousand million years ago."

I trust it may not be presumptuous in me to criticise the views of my great master, at whose intuitive perception of truth in physical questions I have often marvelled, but this passage does not even yet seem to me to allow a sufficiently large margin of uncertainty.

It will be observed that the argument reposes on our certainty that the earth possesses rigidity of such a kind as to prevent its accommodation to the figure and arrangement of density appropriate to its rotation. In an interesting discussion on subaërial denudation Croll has concluded that nearly one mile may have been worn off the equator during the past 12,000,000 years, if the rate of denudation all along the equator be equal to that of the basin of the Ganges ("Climate and Time," 1885, p. 336). Now, since the equatorial protuberance of the earth when the ellipticity is $\frac{1}{230}$ is fourteen miles greater than when it is $\frac{1}{300}$, it follows that 170,000,000 years would suffice to wear down the surface to the equilibrium figure. Now let these numbers be halved or largely reduced, and the conclusion remains that denudation would suffice to obliterate external evidence of some early excess of ellipticity.

If such external evidence be gone,* we must rely on the incompatibility of the known value of the precessional constant with an ellipticity of internal strata of equal density greater than that appropriate to the actual ellipticity of the surface. Might there not be a considerable excess of internal ellipticity without our being cognizant of the fact astronomically?

And, further, have we any right to feel so confident of the internal structure of the earth as to be able to allege that the earth would not through its whole mass adjust itself almost completely to the equilibrium figure?

Tresca has shown in his admirable memoirs on the flow of solids that when the stresses rise above a certain value the solid becomes plastic, and is brought into what he calls the state of fluidity. I do not know, however, that he determined at what stage the flow ceases when the stresses are gradually diminished. It seems probable, at least, that flow will continue with smaller stresses than were initially necessary to start it. But if this is so, then, when the earth has come to depart both internally and externally from the equilibrium condition, a flow of solid will set in, and will continue until a near approach to the equilibrium condition is attained.

When we consider the abundant geological evidence of the plasticity of rock, and of the repeated elevation and subsidence of large areas on the earth's surface, this view appears to me more probable than Sir William Thomson's.

On the whole, then, I can neither feel the cogency of the argument from tidal friction itself, nor, accepting it, can I place any reliance on the limits which it assigns to geological history.

The second argument concerning geological time is derived from the peculiar cooling of the earth.

We know in round numbers the rate of increase of temperature, or temperature gradient, in borings and mines, and the conductivity of rock. These data enable us to compute how long ago the surface must have had the temperature of melting rock, and when it must have been too hot for vegetable and animal life.

Sir William Thomson, in his celebrated essay on this subject (republished in Thomson and Tait's "Natural Philosophy," Appendix D), concludes from this argument that "for the last 96,000,000 million years the rate of increase of temperature underground has gradually diminished from about $\frac{1}{10}$th to about $\frac{1}{50}$th of a degree Fahrenheit per foot. . . . Is not this, on the whole, in harmony with geological evidence, rightly interpreted? Do not the vast masses of basalt, the general appearances of mountain-ranges, the violent distortions and fracture of strata, *the great*

* I find by a rough calculation that $\frac{55}{160}$ths of the land in the northern hemisphere are in the equatorial half of that hemisphere, viz., between 0° and 30° N. lat.; and that $\frac{49}{52}$ths of the land in the southern hemisphere are in the equatorial half of that hemisphere, viz., between 0° and 30° S. lat. Thus for the whole earth, $\frac{104}{212}$ths of the land lie in the equatorial half of its surface, between 30° N. and S. lat. In this computation the Mediterranean, Caspian and Black Seas are treated as land.

prevalence of metamorphic action (which must have taken place at depths of not many miles, if so much), all agree in demonstrating that the rate of increase of temperature downwards must have been much more rapid, and in rendering it probable that volcanic energy, earthquake shocks, and every kind of so-called plutonic action, have been, on the whole, more abundantly and violently operative in geological antiquity than in the present age?"

Now, while I entirely agree with the general conclusion of Sir William Thomson, it is not unimportant to indicate a possible flaw in the argument. The flaw will only be acknowledged as possible by those who agree with the previous criticism on the argument from tidal friction.

The present argument as to the date of the consolidation of the earth reposes on the hypothesis that the earth is simply a cooling globe, and there are reasons why this may not be the case. The solidification of the earth probably began from the middle and spread to the surface. Now is it not possible, if not probable, that, after a firm crust had been formed, the upper portion still retained some degree of viscosity? If the interior be viscous, some tidal oscillations must take place in it, and, these being subject to friction, heat must be generated in the viscous portion; moreover the diurnal rotation of the earth must be retarded. Some years ago, in a paper on the tides of a spheroid, viscous throughout the whole mass (*Phil. Trans.*, part ii, 1879), I estimated the amount and distribution of the heat generated whilst the planet's rotation is being retarded and the satellite's distance is being increased. It then appeared that on that hypothesis the distribution of the heat must be such that it would only be possible to attribute a very small part of the observed temperature gradient to such a cause. Now, with a probable internal constitution for the earth in early times, the result might be very different. Suppose, in fact, that it is only those strata which are within some hundreds of miles of the surface which are viscous, whilst the central portion is rigid. Then, when tidal friction does its work the same amount of heat is generated as on the hypothesis of the viscosity of the whole planet, but instead of being distributed throughout the whole mass, and principally towards the middle, it is now to be found in the more superficial layers.

In my paper it is shown that with Thomson's data for the conductivity of rock and the temperature gradient, the annual loss of heat by the earth is 1/260,000,000 part of the earth's kinetic energy of rotation.

Also, if by tidal friction the day is reduced from D_0 hours to D hours, and the moon's distance augmented from Π_0 to Π earth's radii, the energy which has been converted into heat in the process is $\left(\frac{D}{D_0}\right)^2 - 1 - 8{\cdot}84\left(\frac{1}{\Pi} - \frac{1}{\Pi_0}\right)$ times the earth's kinetic energy of rotation.

From these data it results that the heat generated in the lengthening of the day from twenty-three to twenty-four hours is equal to the amount of heat lost by the earth, at its present rate of loss, in 23,000,000 years.

Now if this amount of heat, or any sensible fraction of it, was actually generated within a few hundred miles of the earth's surface, the temperature gradient in the earth must be largely due to it, instead of to the primitive heat of the mass.

Such an hypothesis precludes the assumption that the earth is simply a cooling mass, and would greatly prolong the possible extension of geological time. It must be observed that this view is not acceptable unless we admit that the earth can adjust itself to the equilibrium figure adapted to its rotation.

It seems also worthy of suggestion that our data for the average gradient of temperature may be somewhat fallacious. Recent observations (Challenger Expedition) show that the lower stratum of the ocean is occupied by water at near the freezing temperature, while the mean annual temperature of the earth's surface, where the borings have been made, must be at least 30° higher. It does not then seem impossible that the mean temperature gradient for the whole earth should differ sensibly from the mean gradient in the borings already made.

The foregoing remarks have not been made with a view of showing Sir William Thomson's argument from the cooling of the earth to be erroneous, but rather to maintain the scientific justice of assigning limits of uncertainty at the very least as wide as those given by him. Professor Tait ("Recent Advances in Physical Science," 1885) cuts the limit down to 10,000,000 years; he may be right, but the uncertainties of the case are far too great to justify us in accepting such a narrowing of the conclusion.

The third line of argument by which a superior limit is sought for the age of the solar system appears by far the strongest. This argument depends on the amount of radiant energy which can have been given out by the sun.

The amount of work done in the concentration of the sun from a condition of infinite dispersion may be computed with some accuracy, and we have at least a rough idea of the rate of the sun's radiation. From these data Sir William Thomson concludes (Thomson and Tait, "Natural Philosophy," Appendix E):—

"It seems, therefore, on the whole most probable that the sun has not illuminated the earth for 100,000,000 years, and almost certain that he has not done so for 500,000,000 years. As for the future, we may say, with equal certainty, that inhabitants of the earth cannot continue to enjoy the light and heat essential to their life for many million years longer unless sources now unknown to us are prepared in the great storehouse of creation."

This result is based on the value assigned by Pouillet and Herschel to the sun's radiation. Langley has recently made a fresh determination, which exceeds Pouillet's in the proportion of

eight to five.* With Langley's value Thomson's estimate of time would have to be reduced by the factor five-eighths.

It has been suggested by Croll that the primitive solar nebula may have been hot. This heat must have arisen from the collision of two or more masses; if their relative velocity before collision was that due simply to their mutual attraction, the heat so generated is already counted in the heat generated by the concentration of the sun from a state of infinite dispersion. But if the relative velocity existed otherwise than from their mutual attraction, then the total heat in the sun might exceed that due simply to concentration. Sir William Thomson considers the hypothesis very improbable. The term improbability seems, however, almost to lose its meaning in these speculations, and at least we know by the spectroscope that actual nebulæ do consist of incandescent gases.

In considering these three arguments I have adduced some reasons against the validity of the first argument, and have endeavored to show that there are elements of uncertainty surrounding the other two; nevertheless they undoubtedly constitute a contribution of the first importance to physical geology. While then we may protest against the precision with which Professor Tait seeks to deduce results from them, we are fully justified in following Sir William Thomson, who says that "the existing state of things on the earth, life on the earth, all geological history showing continuity of life, must be limited within some such period of past time as 100,000,000 years."

If I have carried you with me in this survey of theories bearing on geological time, you will agree that something has been acquired to our knowledge of the past, but that much more remains still to be determined. Although speculations as to the future course of science are usually of little avail, yet it seems as likely that meteorology and geology will pass the word of command to cosmical physics as the converse. At present our knowledge of a definite limit to geological time has so little precision that we should do wrong to summarily reject any theories which appear to demand longer periods of time than now appear allowable.

In each branch of science hypothesis forms the nucleus for the aggregation of observation, and as long as facts are assimilated and coördinated we ought to follow our theory. Thus even if there be some inconsistencies with a neighboring science we may be justified in still holding to a theory, in the hope that further knowledge may enable us to remove the difficulties. There is no criterion as to what degree of inconsistency should compel us to give up a theory, and it should be borne in mind that many views have been utterly condemned, when later knowledge has only shown us that we were in them only seeing the truth from another side. —*Nature*, Sept. 2.

* Langley (*Ann. Rep. R. A. S.*, 1885) estimates that 3 calories per minute are received by a square centimeter at distance unity. This gives for the total annual radiation of the sun $4{\cdot}38 \times 10^{33}$ calories. Thomson gives as Pouillet's estimate 6×10^{30} times the heat required to raise 1 lb. of water 21° Cels., or $2{\cdot}7 \times 10^{33}$ calories.

2. *On the Genesis of the Elements.* — The address by Mr. CROOKES, as President of the Chemical Section of the British Association at the Birmingham meeting, was a noteworthy one. He selected for his subject "the nature and the probable—or at least possible—origin of the so-called elements." After quoting from Faraday, Spencer, Lockyer, Brodie, Stokes and Graham to show that "the notion, not necessarily of the decomposability, but at any rate of the complexity of our supposed elements, is, so to speak, in the air of science waiting to take a further and more definite development," he goes on to discuss the question whether the elements are absolutely and primordially distinct or whether they may not have been evolved from some few antecedent forms of matter—or possibly from only one such—just as it is now held that all the innumerable variations of plants and animals have been developed from fewer and earlier forms of organic life. Conceding that there is no direct evidence of the transmutation of any supposed "element" of our existing list into another, or of its resolution into anything simpler he passes to consider the indirect evidence, that gleaned from the mutual relations of the elementary bodies. Prout's hypothesis, rendered more probable by the half-multiple suggestion of Clarke, shows us that the accepted elements are not coequal but have been formed by a process of expansion or evolution; and since analogy points out that the atomic weight of helium is less than that of hydrogen, this may be the element whose atomic weight will substantiate Prout's law. Another piece of evidence is the close association in the earth's crust of certain groups of elements, such as nickel and cobalt, the two groups of platinum metals, and the so-called rare earths occurring in gadolinite, samarskite, etc., which become more numerous the closer they are examined. The features recognizable in these elements seem to point to their formation severally from some common material placed in conditions in each case nearly identical. So too the doctrine of compound radicals or pseudo-elements, furnishes a weighty argument in favor of the compound nature of the elements, a point to which Carnelley has called attention. If pristine matter was once in an intensely heated condition and has reached its present state by a process of free cooling, the elements as we now have them, as Mills has suggested, may be the result of successive polymerizations. The heat given out in the act of polymerization reverses to some extent the polymerization itself and so causes a partial return to the previous condition of things. This forward and backward movement, several times repeated constitutes "periodicity." Mr. Crookes then discusses the periodic law by the aid of a diagram modified by himself from that of Emerson Reynolds, by inverting it, by representing the pendulous swing as gradually declining in amplitude according to a mathematical law, and by introducing another half swing of the pendulum between cerium and lead, which not only renders the oscillations more symmetrical, but brings gold, mercury, thallium, lead and bismuth on the

side where they are in complete harmony with members of the foregoing groups. "The more I study the arrangement of this zigzag curve," he says, "the more I am convinced that he who grasps the key will be permitted to unlock some of the deepest mysteries of creation. Let us imagine if it is possible to get a glimpse of a few of the secrets here hidden. Let us picture the very beginnings of time, before geological ages, before the earth was thrown off from the central nucleus of molten fluid, before even the sun himself had consolidated from the original *protyle*. Let us still imagine that at this primal stage all was in an ultra-gaseous state at a temperature inconceivably hotter than anything now existing in the visible universe; so high indeed that the chemical atoms could not yet have been formed, being still far above their dissociation-point. In so far as *protyle* is capable of radiating or reflecting, this vast sea of incandescent mist, to an astronomer in a distant star, might have appeared as a nebula showing in the spectroscope a few isolated lines, forecasts of hydrogen, carbon and nitrogen spectra. But in course of time some process akin to cooling, probably internal, reduces the temperature of the cosmic *protyle* to a point at which the first step in granulation takes place; matter, as we know it, comes into existence and atoms are formed. As soon as an atom is formed out of *protyle* it is a store of energy, potential (from its tendency to coalesce with other atoms by gravitation or chemically) and kinetic (from its internal motions). To obtain this energy the neighboring *protyle* must be refrigerated by it and thereby the subsequent formation of other atoms will be accelerated. But with atomic matter, the various forms of energy which require matter to render them evident begin to act; and amongst others, that form of energy which has for one of its factors what we now call atomic weight. Let us assume that the elementary *protyle* contains within itself the potentiality of every possible combining proportion or atomic weight. Let it be granted that the whole of our known elements were not at this epoch simultaneously created. The easiest formed element, the one most nearly allied to the *protyle* in simplicity is first born. Hydrogen—or shall we say helium—of all the known elements the one of simplest structure and lowest atomic weight, is the first to come into being. For some time hydrogen would be the only form of matter (as we now know it) in existence, and between hydrogen and the next formed element there would be a considerable gap in time, during the latter part of which the element next in order of simplicity would be slowly approaching its birth-point; pending this period we may suppose that the evolutionary process which was soon to determine the birth of a new element would also determine its atomic weight, its affinities and its chemical position. In the original genesis, the longer the time occupied in that portion of the cooling down during which the hardening of the *protyle* into atoms took place, the more sharply defined would be the resulting element; and on the other hand with more irregularity in the

original cooling, we should have a nearer approach to the state of the elemental family such as we know it at present. In this way, it is conceivable that the succession of events which gave us such groups as platinum, osmium and iridium—palladium, ruthenium and rhodium—iron, nickel and cobalt—if the operation of genesis had been greatly more prolonged would have resulted in the birth of only one element of these groups. It is also probable that by a much more rapid rate of cooling elements would originate even more closely related than are nickel and cobalt and thus we should have formed the nearly allied elements of the cerium, yttrium and similar groups; in fact the minerals of the class of samarskite and gadolinite may be regarded as the cosmical lumber-room where the elements in a state of arrested development—the unconnected missing links of inorganic Darwinism —are finally aggregated." The author then goes on to draw from the assumption already made that the original protyle contained within itself the potentiality of all possible atomic weights, the conclusion that our atomic weights represent merely a mean value around which the actual atomic weights of the atoms vary within certain narrow limits and hence that "each well-defined element represents a platform of stability connected by ladders of unstable bodies. In the first accreting together of the primitive stuff the smallest atoms would form, then these would join together to form larger groups, the gulf across from one stage to another would be gradually bridged over, and the stable element appropriate to that stage would absorb as it were the unstable rungs of the ladder which lead up to it. I conceive, therefore, that when we say the atomic weight of, for instance, calcium, is 40, we merely express the fact that while the majority of calcium atoms have an actual atomic weight of 40, there are not a few which are represented by 39 or 41, a less number by 38 or 42 and so on." If so, then might not these heavier and lighter atoms be sorted out by a process resembling chemical fractionation? In answer to this question, Mr. Crookes gives the results of experiments which he has carried on for several years on the separation by fractional precipitation of the earths of samarskite and gadolinite. "Down to a date comparatively recent," he says, "nothing was more firmly fixed in my mind than the notion that yttria was the oxide of a simple body; and that its phosphorescent spectrum gave a definite system of colored bands. Broadly speaking there is a deep red band, a red band, a very luminous citron-colored band, a pair of greenish-blue bands, and a blue band. . . . . That the whole system of bands spelled yttria and nothing but yttria, I was firmly convinced. During the later fractionation of the yttria earths and the continued observations of their spectra, certain suspicions which had troubled me for some time assumed consistent form. The bands which hitherto I had thought belonged to yttria began to vary in intensity among themselves and continued fractionation increased the differences first observed. Whilst I was in this state of doubt and uncer-

tainty and only beginning to see my way towards arranging into a consistent whole the facts daily coming to light, help came from an unexpected quarter. M. de Marignac kindly sent me a small specimen of the earth which he had discovered and provisionally named Yα (now gadolinia). In the radiant matter tube this earth gave a spectrum like that of yttria with the chief characteristic band—the citron band—left out, and with the double green band of samaria added to it. A mixture of 61 parts of yttria and 39 parts of samaria gives a spectrum identical almost to the minutest detail with the spectrum of Yα, except that the citron band is as prominent in it as any other line. Yα consists therefore of samaria with the greenish-blue of yttria and some of the other bands of yttria added to it." "I may aptly call the Yα spectrum," he continues, "my Rosetta stone. It threw a flood of light on all the obscurities and contradictions I had found so plentiful and showed me that a much wider law than the one I had been working upon was the true law governing the occurrence of these obscure phenomena. For what does the spectrum of Yα show? It proves that what I had hitherto thought was one of the chief bands in the yttria spectrum—the citron band—could be entirely removed, whilst another characteristic group—the double green of yttria—could also be separated from the citron." The amount of labor involved in these fractionings was very great, a single point often requiring more than 2,000 fractionations to settle it. "Excessive and systematic fractionation has acted the part of a chemical 'sorting demon,' distributing the atoms of yttrium into several groups* with certainly different phosphorescent spectra and presumably different atomic weights, though all these groups behave alike from the usual chemical point of view. Here then is one of the elements, the spectrum of which does not emanate equally from all its atoms but some atoms furnish some, other atoms others, of the lines and bands of the compound spectrum of the element." To test the question whether nature has ever effected such a separation, Mr. Crookes subjected pure yttria (*pure* in respect to every other known element) from different minerals, to the radiant matter test. Samarskite yttria gave all the constituent atom-bands, deep red, red, orange, citron, greenish-blue and blue in fair proportion, the first being faintest. Gadolinite yttrium gave the citron and greenish-blue constituents plentiful, the red very deficient, the orange absent, the others moderate. Xenotime yttria gave the citron the most abundant, the greenish-blue smaller, the red all but absent, and the orange absent. Monazite yttria gave the greenish-blue and the citron, the other constituents being in fair proportion and the red good. Fluocerite yttria is quite similar, though the blue is weaker. Hielmite yttria is very rich in citron, has a fair quantity of blue and greenish-blue, less of red, no orange and only a

* In a communication to the Section on the fractionation of yttria, Mr. Crookes stated that there are certainly five and probably eight, constituents into which yttrium may be split.

very faint trace of deep red. Euxinite yttria is almost identical with this. Cerite yttria contains most red and citron, less greenish-blue and blue, only a trace of deep red and no orange." "Hence in some, possibly in all elements, the whole spectrum does not emanate from all its atoms, but different spectral rays may come from different atoms, and in the spectrum as we see it all these partial spectra are present together." The address now proceeds further in the order of elemental evolution. In the undulating curve in the diagram may be seen the action of two forces, one acting in the direction of the vertical line and the other pulsating backwards and forwards like a pendulum. The vertical line may be assumed "to represent temperature slowly sinking through an unknown number of degrees from the dissociation point of the first formed element down to the dissociation point of those last shown on the scale. But what form of energy is represented by the oscillating line? Swinging to and fro like a mighty pendulum to points equidistant from a neutral center; the divergence from neutrality conferring atomicity of one, two, three and four degrees as the distance from the center is one, two, three or four divisions; and the approach to or retreat from, the neutral line deciding the electro-negative or electro-positive character of the element—all on the retreating half of the swing being positive and all on the approaching half negative—this oscillating force must be intimately connected with the imponderable matter, essence or source of energy we call electricity. Let us examine this a little more closely. Let us start at the moment when the first element came into existence. Before this time matter as we know it, was not. It is equally impossible to conceive of matter without energy as of energy without matter; from one point of view the two are convertible terms. Before the birth of atoms all those forms of energy which become evident when matter acts upon matter could not have existed—they were locked up in the *protyle* as latent potentialities only. Coincident with the creation of atoms all those attributes and properties which form the means of discriminating one chemical element from another, start into existence fully endowed with energy. The pendulum begins its swing from the electro-positive side; lithium, next to hydrogen in simplicity of atomic weight, is now formed; then glucinum, boron and carbon. Definite quantities of electricity are bestowed on each element at the moment of birth, on these quantities its atomicity depends and the types of monatomic, diatomic, triatomic and tetratomic elements are fixed. The electro-negative part of the swing now commences, nitrogen appears, and notice how curiously position governs the mean dominant atomicity. Nitrogen occupies the position below boron, a triatomic element, therefore nitrogen is triatomic. But nitrogen also follows carbon, a tetratomic body, and occupies the fifth position counting from the place of origin. How beautifully these opposing tendencies are harmonized by the endowment of nitrogen with at least a double atomicity and making its atom

capable of acting as tri- and pentatomic. With oxygen (di- and hexatomic) and fluorine (mon- and heptatomic) the same law holds and one half-oscillation of the pendulum is completed. Again passing the neutral line the electro-positive elements, sodium (monatomic), magnesium (diatomic), aluminum (triatomic), and silicon (tetratomic), are successively formed and the first complete oscillation of the pendulum is finished by the birth of the electro-negative elements, phosphorus, sulphur and chlorine; these three—like the corresponding elements formed on the opposite homeward swing—having each at least a double atomicity depending on position. Let us pause at the end of the first complete vibration and examine the result. We have already formed the elements of water, ammonia, carbonic acid, the atmosphere, plant and animal life, phosphorus for the brain, salt for the sea, clay for the solid earth, two alkalies, an alkali-earth, an earth, together with their carbonates, borates, nitrates, fluorides, chlorides, sulphates, phosphates and silicates, sufficient for a world and inhabitants not so very different from what we enjoy at the present day. True the human inhabitants would have to live in a state of more than Arcadian simplicity and the absence of calcic phosphate would be awkward as far as bone is concerned. But what a happy world it would be! No silver or gold coinage, no iron for machinery, no platinum for chemists, no copper wire for telegraphy, no zinc for batteries, no mercury for pumps and alas! no rare earths to be separated." After a discussion of the process as it continues to form the other elements, and the consequences naturally resulting therefrom, the author says: "Summing up all the above considerations, we cannot indeed, venture to assert positively that our so-called elements have been evolved from one primordial matter; but we may contend, I think, that the balance of evidence fairly weighs in favor of this speculation. —*Nature*, xxxiv, 423–432, Sept. 2, 1886.

3. *On Austrium, a new metallic element.*—LINNEMANN has announced the discovery of a new metallic element in the orthite from Arendal to which he has given the name Austrium. To obtain it the mineral is treated with hydrochloric acid, the solution treated with hydrogen sulphide to remove the lead, copper, tin and arsenic, the filtrate nearly neutralized or mixed with sodium-acetate and hydrogen sulphide again passed through it. The precipitate now obtained contained copper, lead, zinc, cadmium, thallium, iron, calcium, magnesium and austrium, these metals being detected by the spark spectra of their chloride solutions. This precipitate was dissolved in hot hydrochloric acid, the excess of acid evaporated, the solution treated with caustic soda in excess, filtered, the filtrate precipitated after warming, with sodium sulphide, the precipitate filtered off and the filtrate allowed to stand exposed to the air. The austrium which it contains is thus converted into carbonate and partly precipitated, mixed with sulphur. On acidifying the filtrate with acetic acid, evaporation to dryness, solution in water and treatment with

hydrogen sulphide, nearly all remaining in solution is thrown down mixed with traces of copper, lead and zinc, separation from the latter being especially difficult. The spark spectrum of the metal austrium is characterized by two violet lines; one of these Aus α has a wave length 4165, the other Aus β has a wave length of 4030. The former appears to coincide with the solar line 4164·7 of Ångström.—*Monatsch. Chem.*, vii, 121; *Ber. Berl. Chem. Ges.*, xix, 431, July, 1886.

4. *On Holmium, Dysprosium and Gadolinium.*—Lecoq de Boisbaudran has submitted the earth called X by Soret, to a hundred fractionings by means of ammonia and potassium sulphate, and has separated it into two portions. For one of these, whose absorption spectrum exhibits the bands 640·4 and 536·3, he proposes to retain the name holmium; while for the other, containing the bands 753 and 451·5 in its spectrum, he suggests the name Dysprosium (from δυσπρόσιτος, difficultly accessible). Subsequently he gives 804 (?) 756·5, 475, 451·3 and 427·5 as the bands of dysprosium. In a letter to him, Marignac gives the name Gadolinium to the element he has before indicated by Yα. —*C. R.*, cii, 902, 1003, 1005; *Ber. Berl. Chem. Ges.*, xix, 388 (Ref.); *J. Chem. Soc.*, l, 667, August, 1886.

II. Geology and Natural History.

1. *Geological Nomenclature: Meeting of the International Committee at Geneva.*—At the meeting of the committee of the International Geological Congress on geological nomenclature, at Geneva, in August last, Messrs. Capellini, the President, Dewalque, Lapparent, Renevier and Vilanova were present. A proof sheet of the proceedings, yet unfinished, has been received from Mr. Dewalque, the secretary of the committee, and from it the following abstract has been made out.

The question as to the terms Silurian and Cambrian was, by request from England, left for the next general session.

The terms for the higher subdivisions were considered and the following agreed upon, commencing with the highest: (1) *Group*: as, for example, the Primary, Secondary and Tertiary, corresponding to Paleozoic, Mesozoic and Cenozoic.—(2) *Sub-group:* as the Jurassic, Cretaceous.—(3) *System:* as the Liassic.—(4) *Stage* (étage): as the Sinemurian.—(5) *Sub-stage.*

The word *series* was reserved for eruptive rocks, and for use in a general way. For chronological terms, it was decided, on the suggestion of M. Lapparent, that the correlate of Group should be *Era;* of Sub-group, *Time;* of System, *Period;* of Stage, *Age;* of Sub-stage, *Epoch.* By this scheme we should have, for example, the *Mesozoic era, Jurassic time, Liassic period, Sinemurian age*, and under the age, *epochs.* Renevier suggested the use of *sub-era* in place of *time*, but Lapparent's proposition was carried.

A third point considered was the termination of the adjective terms used to indicate subdivisions under the above heads. M. Lapparent thought that it was not best to make any definite proposal at present, on the ground that terms in use were well understood, and the probability of having to modify again. But M. Renevier proposed the following scheme, which was adopted: that the adjective terms of a Group, should have the termination *ary* (*aire* in French) as Primary, etc.; of a System, *ic* (*ique*, Fr.); of a Stage, *ian* (*ien*, Fr.); of a Sub-stage, *ine* (*in*, Fr.).

The following points referred to the committee were answered as follows:

The divisions of highest rank should be based on paleontological characters of universal or world-wide application. The Systems should be marked off by comprehensive facts in paleontology indicative of the general march of progress, especially facts that relate to pelagic species. The common characters of the Systems will necessarily serve for the defining of the Sub-groups. The Stages should be characterized by distinct pelagic faunas; and the Sub-stages should have a regional value. Inferior divisions, having only a local value, were regarded as not coming under the consideration of the International Congress. The committee also decided that a stratigraphic argument needed always to be confirmed by paleontological evidence. J. D. D.

2. *Geological Age of the North Atlantic Oceanic basin and Origin of Eastern American sediments.*—In *Nature*, of Sept. 23, Professor Edward Hull, of Dublin, objects to a statement in President Dawson's Address in which the latter says: "I prefer, with Hall, to consider these belts of sediments as, in the main, the deposits of northern currents, and derived from Arctic land, and that, like the great banks of the American coast of the present day, which are being built up by the present Arctic currents, they had little to do with any direct drainage from the adjacent shore," Professor Hull observing rightly that the deposits spread widely over the continent without reference to the shore line, and that the currents are inadequate in other respects. Mr. Hull states the view he has held, not that the Arctic lands were the source, but the region of the Atlantic, "towards which the sediments thicken, and opposite to that in which the limestones are most developed;" and thence, that the conclusion is inevitable "that the Atlantic was in the main a land-surface in Paleozoic times."

Professor Hull misunderstands American geology in an important respect. The Upper Silurian and Devonian deposits, which have so great thickness to the eastward, stretch northward over central and southern New York, to a large extent behind or west of the Green Mountain region, which was already in shallow water, and probably emerged land; and the Carboniferous formation extends up in the same direction, though to a less distance. Further, the Appalachian Paleozoic beds from their beginning were laid down to a very great extent to the west of the Archæa

highlands, those of the chief part of the Blue Ridge, of the Highlands of New Jersey and New York, and of various scattered areas over the higher parts of western New England. If these Archæan lands were largely covered at the close of the Paleozoic era—not a probable supposition for many parts—they were certainly not so during the earlier part, the deposits of which are now below the level of the Archæan lands.

Such facts show that neither the Arctic regions nor any Atlantic continent could have contributed essentially to the growing deposits. Further, the latest results of study among these older deposits satisfy the writer, as it has others, that the origin of the material can be found in the adjoining Archæan of the Eastern Border region; and that the sand reefs and some of the shore lines of the successive periods will be more and more thoroughly traced out over the continent as investigation goes forward: work that was begun and carried well forward when the Professors Rogers, in Pennsylvania and Virginia, and Emmons, Hall, Vanuxem and Mather in New York, made their early investigations nearly fifty years since. J. D. D.

3. *Annual Report of the Geological Survey of Pennsylvania for* 1885; by the State Geologist. 770 pp. 8vo, with 18 plates and many wood-cuts, and an atlas of 8 folded sheets.—The general introduction to this Report by the State Geologist, Professor Lesley, has been noticed on page 162 of this volume. Following this, the volume contains a Preliminary report on Oil and Gas, by JOHN F. CARLL; on the vegetable origin of Coal, by L. LESQUEREUX; on the re-survey of the Pittsburgh Coal-region; by E. V. D'INVILLIERS; on the Coal-beds and fire-clays of the Wellesburg Basin, by J. P. LESLEY, with a special report on fire-clay, by E. B. HARDEN; on the Coal-bed in the Pocono formation, No. X, Second Report of Progress in the Anthracite Coal-region, Part II, on the Brandywine Summit Kaolin beds, and on the Archbald pot-holes, by C. A. ASHBURNER; on the fossils of the Wyoming Valley limestone beds, by A. HEILPRIN; on the Cornwall ore mines, Lebanon Co., by J. P. LESLEY and E. V. D'INVILLIERS; on the origin and distribution of the Delaware and Chester Kaolin deposits, by J. P. LESLEY; General Considerations of the pressure, quantity, composition and fuel value of Rock-gas of Pennsylvania, by J. P. LESLEY; on the buried Wyoming Valley, by F. A. HILL; on a substance resembling dopplerite, by H. C. LEWIS; Progress of the geodetic triangulation of Pennsylvania, by M. MERRIMAN.

The reports in this volume present a general review of many subjects connected with the geology of Pennsylvania, in order to exhibit especially, the great value and extent of the work done, correct popular errors by full explanations, and give general results in connection with the coal, oil, gas, fire-clays, and other products studied by the Survey. Moreover, figures are given of many marine fossils from limestone beds in the coal formation, and a considerable number from the Wyoming anthracite region. It is

thus an excellent volume for the State, and a valuable one for the geologist. Its many maps and other illustrations make it easily intelligible and supply much information to the eye. Mr. Ashburner's chapter on the anthracite and other regions show briefly what has been accomplished, and reviews in an instructive way the composition and fuel value of the coal. A very interesting chapter by Lesley and d'Invilliers, gives an account of a bed of magnetite in the Triassic region of Cornwall, in Lebanon County, 300 feet below the surface and nearly 63 acres in estimated area, which became magnetite from limonite by the heat of a trap eruption, and whose original limonite was made through the decay of a ferriferous limestone of the Triassic formation.

The paper by Professor Lesley on rock-gas of western Pennsylvania, its origin, pressure, quantity, composition and fuel-value, supplements the important chapter by Mr. Carll, explaining many points about which there is much popular misunderstanding, and considering from a basis of facts the probable future of gas-production in Pennsylvania, foretelling its rather speedy exhaustion.

Another paper by Professor Lesley, on the Delaware and Chester Kaolin beds, endeavors to explain the origin of these deposits by reference to the known or the probable position of gneiss ridges below the Paleozoic, and the subject leads to remarks on the great amount of denudation in past time over the region, through all periods to the present, and on the origin of kaolin beds generally. As the kaolin beds are beneath or near limestone belts the decay of the limestone, it is suggested, may have helped forward by its liberated carbonic acid and in other ways, the decay of the underlying gneisses. The necessity of favorable conditions for the percolation of waters by opportunities for natural drainage is dwelt upon. The subject is an interesting one and the discussion instructive.

4. *Catalogue of the Blastoidea in the Geological Department of the British Museum*, by ROBERT ETHERIDGE, Jr., of the Department of Geology, British Mus., and HERBERT CARPENTER of Eton College. 322 pp. 4to, with 20 plates. London, 1886.—This "catalogue" contains a very full account of the morphology of the Blastoidea, a discussion of the systematic position of the group, and a revision of the genera and species, with descriptions and illustrations on the plates of their structure and of the species mentioned. The authors are the highest British authority on the subject and understand thoroughly the morphology of Echinoderms. Their work is therefore one of high authority. Mr. Chas. Wachsmuth has been over the same ground, as regards the subject of structure, with like ability, and with the aid of the large diversity of American forms. The authors say in their preface, "our chief difficulty, the want of adequate material, was soon and simply solved; for Mr. Charles Wachsmuth, of Burlington, Iowa, whose admirable work on the Palæocrinoids is known to every palæontologist, generously offered to place at our disposal a selected

series from his fine collection of American Blastoids;" and the collections "lent for six months only," through the owner's liberality remained "for over five years." "Mr. Wachsmuth has also been kind enough to keep us informed of the progress of his own researches, and in some instances these have led to the abandonment or modification of views which we had previously expressed." Messrs. Etheridge and Carpenter differ in some points from Mr. Wachsmuth, "but it is perhaps scarcely to be expected that there should be a complete concordance of opinion between workers who approach the subject from altogether different sides." The authors, besides, express their indebtedness to several other American workers, as also to many in Europe.

The British Museum collection of Blastoids "is not likely to be surpassed in richness," many American species being added to the British, and others from various parts of Europe. All the nineteen genera are represented in it excepting the rare *Pentephyllum* and *Eleutherocrinus*. Of the former, only one specimen (now in the museum of the University of Dublin) is known.

5. *Revision of the Palæocrinoidea*, by CHARLES WACHSMUTH and FRANK SPRINGER. Second Section of Part III. (From the Proc. Acad. Nat. Sci., March, 1886.)—This concluding portion of Part III of Mr. Wachsmuth's important work, covers 194 pages. The subject is the "Discussion of the classification and relations of the Brachiate Crinoids and conclusion of the generic descriptions."

6. *A Natural System of Mineralogy with a Classification of native Silicates;* by THOMAS STERRY HUNT (reprinted from Mineral Physiology and Physiography, pp. 279–401. Boston: S. E. Cassino).—In this essay, which has been already presented to the public in full or in abstract in several publications, Dr. Hunt discusses the general question of mineral classification, and gives the special application to the silicates of the views urged by him.

The subject is too broad to be fully discussed here, but briefly it may be stated that Dr. Hunt urges that in classification not only the chemical composition should be taken into account but the physical characters as well, and especially the specific gravity and hardness. The point made by Dr. Hunt that the hardness and specific gravity, and with them the degree of resistance to attack by solvents, are highly significant as showing the degree of atomic condensation, and hence indicating what species are allied to each other, is one of importance which has been too little recognized.

His method of calculation is applied to the silicates and the results taken in connection with crystallization, hardness and degree of resistance to acids serve to give the basis of the classification. The silicates are thus divided into three sub-orders: (1) those containing no alumina, (2) those with alumina and protoxides, (3) those with alumina and without protoxides. Each of these is separated into five tribes passing from the species of inferior hardness, lower density and easily decomposed by acid to those

which are high in hardness and density and not readily decomposed, and then to the softer foliated "phylloid" species, and finally those which are non-crystalline and colloidal. For the details reference must be made to the original paper. In regard to the general result of such a classification it may be said that it for the most part, as was to be expected, serves to bring species together whose relationship has long been recognized, at the same time there are notable exceptions, as when spodumene, which was commonly grouped with the pyroxenes with which it agrees in form, is separated and finds a place beside staurolite. Other cases could be mentioned, but the relations brought out are in any case suggestive and of value.

It is not clear, however, that the chemical basis of the subdivision of the silicates into the three groups mentioned is a necessary consequence of the special principles laid down by Dr. Hunt.

7. *Flowers, Fruits, and Leaves;* by SIR JOHN LUBBOCK, Bart., etc.—In the *Nature Series* of Macmillan & Co., 1886, the articles which were first given as popular lectures and afterwards published in *Nature*, have been collected into this small and very readable volume. That is, to the former Lectures on Flowers—which were published several years ago, at a time where Hermann Muller's writings on the relations of common flowers to Insects were not otherwise very accessible to English and American readers—are now added two equally popular later lectures on fruits and seeds, and on leaves. In all, the structures and forms are especially considered in reference to their utilities, "opening out a very wide and interesting field of study." As the author was not bred to botany and is much given to a particular set of ideas, he now and then runs some risks, but is generally trustworthy as well as interesting in his favorite line of exposition. A. G.

8. *Lectures on the Physiology of Plants;* by SYDNEY HOWARD VINES, M.A., D.Sc., F.R.S., Fellow and Lecturer of Christ's College, Cambridge, and Reader in Botany in the University. Cambridge: University Press, 1886, pp. 710, 8vo.—This handsomely printed and stout volume presents a clear and well arranged and well proportioned summary of vegetable physiology, as it has become under the hands of its productive votaries within the last dozen or so of years. For advanced students this book should accompany and supplement the physiological part of Professor Goodale's Text Book, being naturally fuller in treatment, especially complete in bibliographical references, and entering with much particularity into the discussion of certain rather theoretical questions connected with cell-structure and reproduction. We are thus provided with good abstracts of the present state of our knowledge of the subject, in our own vernacular, and in a presentation which for lucidity and succinctness may not always be found in the German sources or in translations from them. The great advance in vegetable anatomy and physiology of late years naturally is due to the special attention which has been given to them by a numerous class of phytologists, especially on the Euro-

pean continent, who are mainly dissociated from taxonomical botany and morphology. Hence a certain looseness and redundance in terminology, and now and then such confusion as the author of these lectures refers to in his preface,—where we learn that the physiologists speak of the normally lower surface of a leaf as ventral and of the upper as the dorsal, a sort of "*nous avons changé tout cela*" which will astonish the morphologists. A. G.

9. *A Preliminary Synopsis of North American Carices, including those of Mexico, Central America, and Greenland, with the American Bibliography of the Genus;* by L. H. BAILEY, JR., is the modest title of a very elaborate memoir of about 100 pages, just issued as a separate edition of a paper published by the American Academy of Arts and Sciences, in the 22d volume of its Proceedings. It is headed as a "Contribution from the Herbarium of Harvard University," where much of the work was done. It will be prized by our numerous botanists who take a critical interest in the vast genus Carex. The author is one of the professors of the State Agricultural College of Michigan.

III. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *British Association in Birmingham.*—An unusual feature of the British Association this year was the occupancy of the Presidential chair by a member from Canada, Sir J. WM. DAWSON, of Montreal. The inaugural address of the President recognizes in the fact an augury of future international union in scientific work, if not an International Association, remarking that "As a Canadian, as a past President of the American Association and now honored with the Presidency of this Association, I may be held to represent in my own person the scientific union of the British Islands, of the various Colonies and of the great Republic, which, whatever the difficulties attending its formal accomplishment at present, is certain to lead to an actual and real union for scientific work." The address was devoted to various questions in geological science under the general subject of the history (including the permanency or not) of the Atlantic Ocean basin. Other opening addresses were by Professor G. H. DARWIN in the Mathematical and Physical Section; Professor WM. CROOKES, in the Chemical; Professor T. G. BONNEY, in the Geological (relating especially to Britain's Geological history); Professor WM. CARRUTHERS, in the Biological; Sir GEORGE CAMPBELL, in the Anthropological; Sir F. J. GOLDSMID, in the Geographical, and Sir JAMES N. DOUGLASS in the section of Mechanical Science. These able Addresses, together with reports of the work of Committees and Sections, will be found in the numbers of *Nature* for the month of September, commencing with No. 879, or that for September 2d.

OBITUARY.

Colonel CHARLES WHITTLESEY died in Cleveland, Ohio, October 18th, two weeks after entering on his seventy-ninth year. A notice of his geological and other work is deferred.